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VOL. VI, NO. 2 JUNE, 1931

THE QUARTERLY REVIEW
of BIOLOGY

ANALYSIS OF INTERSEXUALITY IN THE GIPSY-MOTH

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DURING the past twenty years I have, with a number of collaborators, analyzed the phenomenon of intersexuality in the gipsy-moth, *Lymantria dispar* L. A few smaller contributions have been added by such authors as Kosminsky, Lenz, Schweitzer, Standfuss. This work is now practically finished, at least as far as I am concerned, and I believe that every important point has been sufficiently worked out, though minor gaps are certainly left. The long time over which this work extended necessarily led to certain changes in our views, as work progressed, as well as to corrections of experimental data with increasing bulk of evidence—both of which make the study of the original papers still more laborious than is already the case on account of the mass and complexity of the facts. A review of the whole work, as it stands to-day is therefore probably not out of place. The innumerable moths which have been bred in these experiments have been preserved and are available for study or control in our laboratory. The genetic side of the work will be reviewed more thoroughly than the more easily accessible morphological and general work.

INTRODUCTION

Intersexuality in the gipsy-moth is a phenomenon which occurs entirely within the normal chromosome behavior. It might therefore be described as diploid intersexuality. (For exceptions see later, non-disjunction.) The gipsy-moth shows like other Lepidoptera the Abraxas type of sex-chromosomes (female heterogamety) which is clearly demonstrated by the intersexuality experiments. Cytologically an X-Y group could thus far not be distinguished and among the mutations studied by me and Machida no sex-linked character appeared. In the next relative of the gipsy-moth, the nun moth *Lym. monacha*, an X-Y group exists in the female (Seiler) and sex-linked inheritance of the Abraxas type was found (Goldschmidt). Intersexuality is produced in an absolutely orderly way by crossing different geographic races of this moth. Occasionally intersexual specimens are found in nature within a pure race and recently *Kosminsky* (52) has discovered and studied

a case where within a pure Russian race lines could be isolated which regularly threw intersexes. We shall refer later to this exceptional phenomenon, which had never occurred among the pure strains of numerous races which I bred in innumerable numbers.

The intersexual individuals or intersexes are of two main types, which are called female and male intersexes respectively. These names are convenient though not logically correct. What is meant is that female intersexes are XY-individuals which ought to be females, if only the chromosome set were decisive; male intersexes are correspondingly XX-individuals. Intersexes represent macroscopically a definite stage between the two sexes and male and female intersexes can be distinguished at first sight. Closer inspection shows that an intersex represents a strange mixture of the individual female and male characters, which are combined in a certain orderly way. Therefore in the beginning of our work (1911–15) we used the term gynandromorphs for these types, though emphasizing their difference from typical gynanders. Since 1915 we introduced the term intersexes and clearly defined the difference between gynandromorphism and intersexuality. Female as well as male intersexes in different types appear again according to definite laws representing the different degrees of intersexuality, characterised by the relative amounts of female and male structures. Thus a series of female intersexuality can be found beginning with normal females and leading through increasing grades of intersexuality (increasing of male admixtures) to complete sex-reversal i.e. normal functioning males of XY-constitution. A corresponding series of male intersexuality leads from a normal male through all the grades of male intersexuality to a sex-reversal—female. The simple law which explains the structure of these different intersexes and which had first been noticed by Baltzer in the intersexes of *Bonellia*, is this: an intersex is an individual, which is developed first with its gametic (XX or XY) sex but from a certain moment in development, the turning point, completes its development with the other sex. The degree of intersexuality is therefore a function of the position of the turning point in time: earlier turning point—higher degree of intersexuality. The phenomenon of intersexuality affects all sexual characters i.e. primary and secondary sex characters—simultaneously according to the forementioned law. (In my first papers (1911, 1912) I thought that only secondary characters were involved; later I could show that the primary characters were also affected simultaneously.) Therefore only general sex-genes are involved in the analysis.

THE BASIC EXPERIMENTS

Reciprocal crosses between a certain European and a certain Japanese race (which one see later) give the following results:

1. European female × Japanese male:
   All sons normal, all would-be daughters intersexual.
2. Japanese female × European male:
   Sons and daughters normal.
3. F_2 from Nr. 1 (only possible, if lowest type of intersexuality, which alone is fertile, is obtained):
   All sons normal, daughters ½ normal, ½ intersexual, intersexuality of the same degree as in the mother.
4. F_2 from Nr. 2:
   All daughters normal, sons at least ½ normal, up to ½ intersexual (for this ratio see later).

This experiment leads to a convenient terminology which is free from theoretical assumptions and will be used henceforth. The Japanese race of this first experiment

is called strong and the European race weak. Female intersexuality is then produced in F_1 if a weak female is crossed with a strong male; male intersexuality appears in a part of the gametic males in F_2 of the reciprocal cross. The first experiment already shows that both sexes contain the anlagen for either sex, which can be brought to light in definite gametic combinations. It further shows that the resulting sex (including intersexuality) is dependent upon two genetic somethings, one of which shifts sex towards the female, the other towards the male side, or in other words female and male determiners. It further shows that sex is dependent upon a certain relation of these determiners, their relative strength or weakness, in other words upon a balance or unbalance of these determiners. Thus even in my first publication on this subject in 1911, the conception was introduced, which is nowadays called genic balance.

The next question is how are these strong and weak sex determiners inherited and how are the different results of reciprocal crosses to be explained? The answer, which later will be verified in most decisive tests, was derived first from the following elementary set of F_1 and F_2 combinations.

1. Crosses within the maternal weak line

COMBINATION	RESULT	
F_1 weak ♀ × strong ♂...	all ♀ ♪ (intersexual)	all ♂ normal
F_2 (weak ♀ × strong ♂)².	¼ ♀, ¼ ♀ ♪	
backcross: weak ♀ × (weak ♀ × strong ♂) ♂.	¼ ♀ ¼ ♀ ♪	
backcross: weak ♀ × (strong ♀ × weak ♂) ♂.	¼ ♀ ¼ ♀ ♪	
backcross: (weak ♀ × strong ♂) ♀ × weak ♂..	all ♀ normal	
backcross: (weak ♀ × strong ♂) ♀ × strong ♂.	all ♀ ♪	

In all these crosses the maternal line is weak. The results show at once that the "strength" which produces female intersexuality is inherited within the X-chromosomes. We do not use the terms W and Z chromosomes in the case of female heterogamety. One of the reasons is the following: We can produce sex-reversal-males which are heterogametic, and sex-reversal-females which are homogametic. Are their respective sex-chromosomes now XY or WZ? Each female receives her single X-chromosome from the father. The results are exactly what is to be expected if this intersexuality-producing strength is a property of the X-chromosome of the strong race. It shows further that correspondingly the X-chromosome of the weak race carries the property weakness. It finally shows that the F_1 females behave like pure weak females in the hybrid combination (weak ♀ × strong ♂) × strong ♂, that is in the maternal weak line.

2. Crosses within the maternal strong line

COMBINATION	RESULT	
F_1 strong ♀ × weak ♂.....	normal ♂	all ♀ normal
F_2 (strong ♀ × weak ♂)².....	¼ ♂ ¼ ♂ ♪*	
backcross: strong ♀ × (strong ♀ × weak ♂) ♂.....	normal ♂	
backcross: strong ♀ × (weak ♀ × strong ♂) ♂.....	normal ♂	
backcross: (strong ♀ × weak ♂) ♀ × strong ♂.....	normal ♂	
backcross: (strong ♀ × weak ♂) ♀ × weak ♂.....	all ♂ ♪*	

* For exact ratio see later.

This series shows: The property "weakness" of the weak males is inherited in their X-chromosomes; in these combinations two strong X-chromosomes or one strong and one weak X-chromosome produce normal ♂♂; two weak X-chromo-

somes produce intersexual ♂♂. All mothers behave as strong females, never producing female intersexuality, whether they are of pure strong race or F_1 hybrids between a strong mother and a weak father.

If we consider both series together we realize that strength and weakness are, first, properties of the X-chromosomes of the respective races and, second, properties of something inherited in the female line, i.e. maternally only; further that the resulting sex is determined by a definite relation or balance between the respective strength (or weakness) of the one type of determiners inherited maternally and the other type inherited within the X-chromosomes; further that, whereas the combination of the weak maternally inherited thing with a strong determiner in an X-chromosome shifts the would-be female towards maleness (female intersexuality) and whereas the combination of a strong maternally inherited thing with two weak determiners in the X-chromosomes shifts the would-be male towards femaleness (male intersexuality), the X-chromosomes must contain a male determiner, strong in the strong and weak in the weak races, and that the thing which is inherited maternally is a female determiner, again strong in the strong and weak in the weak races. This means that sex in the gipsy-moth is determined by a relation or balance between a maternally inherited female determiner, F, and one (♀) or two (♂) male determiners, M, inherited within the X-chromosomes. In Mendelian symbols, $\frac{F}{M\ (X)} = ♀$, $\frac{F}{M\ (X)\ M\ (X)} = ♂$. It further means that the pure sexes appear, if F and M or MM are properly balanced or, in a formula, $\frac{F}{M} = \text{min} = ♀$, $\frac{MM}{F} = \text{min} = ♂$; that intersexes appear if by hybrid combination of F and M of not matched relative strength F and M are not properly balanced, or in a formula $\frac{F}{M}$ = less than min = ♀ J, $\frac{MM}{F}$ = less than min = ♂ J. The minimum required to insure the complete superiority of M or F over its counterpart F or M has been termed epistatic minimum. Between the two epistatic minima $\frac{F}{M} = +e$ and $\frac{F}{MM} = -e$ the series of intersexes are situated, female intersexes in the XY animal, male intersexes in the XX animal. The results of this analysis of the elementary set of experiments have been tested with absolutely positive results in the further work.

ARE F AND M SINGLE GENES?

Already at this point of the analysis the question might be asked, whether F and M have to be considered as single genes. The answer is that never in the thousands of crosses, partly of the most complex type, was any result obtained, which would support any other conclusion; further that numerous multiple allelomorphs of F and M have been studied and that mutations of the "strength" of these genes have been observed, (see 25). Under these circumstances F and M have to be treated as single genes, if we keep within the elementary rules of genetic analysis. Occasional utterances of critics that they do not "believe" it may therefore be disregarded. But it might be worth while to uncover the basis of such beliefs, because it is found in a widespread misconception of what sex-determination means. The idea is that sexes are distinguished by so many different morphological and physiological characters that such differences could not be accounted for by a single gene difference. Such a view reveals in my opinion very loose thinking,

disregarding completely the difference between sex inheritance and other Mendelian inheritance. Every individual of a form which exhibits sexual differences, contains the necessary genetic outfit for the production of the characters of both sexes, an obvious fact, but finally proven by the phenomenon of diploid intersexuality. But in development a given group of cells, say the primordial gonocytes, or the anlagen of an antenna, has at a certain moment to decide which of the alternative possibilities of differentiation, present as "*reactionsnorm*" on the basis of the whole genetic constitution, will be realized. This decision is brought about by the relative condition (balance) of the F and M genes at the moment of deciding the alternative. Intersexuality definitely shows that this decision may be reversed in the same individual at consecutive moments. The viewpoint which we are discussing obviously mistakes the set of general genes which have an alternative *reactionsnorm*, according to the $\frac{F}{M}$ situation, for sex-determining genes. The error is the same as if somebody should conclude from the fact that a colored Primula has a white flower at high temperature that more than one gene was involved. The difference between the two examples is of course that in *Primula* the factors which decide the alternative are external factors, in the case of sex, however, internal factors on a genetic basis. But logically both cases are alike. Concerning modifying genes see later.

DIFFERENT TYPES OF STRONG AND WEAK RACES AND THE MULTIPLE ALLELOMORPHS OF F AND M

Some results obtained in repeating the first experiments led to the expectation that different strong and weak races were to be found in different geographical habitats, especially in Japan. Repeated collecting trips confirmed this expectation and the analysis of these different races has not only furnished extremely interesting results for the problem of geographic variation, which will not be discussed here, but also permitted a complete analysis of intersexuality. Races over the whole range of distribution of the gipsy-moth, i.e. over the range of the palaearctic region, many of which can also be distinguished by somatic characters, have been tested (see 41). According to their behavior in the intersexuality experiments the following types may be distinguished: 1. *Strong races.* These have strong F and M. A strong male crossed to a weak female produces in F_1 normal ♂♂ and female intersexes. The degree of intersexuality is typical for a given cross and different with different combinations of the individual strong and weak races. F_2 of the reciprocal cross throws a certain percentage (up to 50%) of male intersexes. The strong races are exclusively found in the mainland of North-Eastern Japan, the area being practically identical with that natural division of Japan which is called Kwanto. There are indications that in the mountainous districts different forms occur, but the decisive tests are not yet finished. Within this area the strength of F and M is not constant. The different tests show that generally speaking the "strength" decreases from Northeast to Southwest. A number of strong races of different strength have been tested, which e.g. means that one and the same type of weak female produces increasing grades of female intersexuality in F_1 with males of increasing strength. These results are typical and have never failed when repeating the respective crosses. 2. *Neutral races.* A female of a neutral race if crossed to a male of any race, weak or strong, produces in F_1 normal offspring.

This shows the F of the neutral race to be relatively strong. A male of a neutral race produces with all females, even with those of the weakest races, only normal offspring. This shows the M of the neutral race to be relatively weak. F_2 of the cross neutral ♀ × weak ♂ produces a certain percentage of male intersexes, again showing the F of the neutral race strong. It will later be shown how these different strengths may be measured. The neutral races inhabit all south-western Japan, the region called Kwansai, and also the southern Island of Kyushiu (see later). Again the strength of their F and M decreases from East to West, the strongest neutrals being found near the borderline between Kwanto and Kwansai, the weakest in the island of Kyushiu. (For tests see later). Neutral races are further found in Northern Korea, Manchuria, North-Eastern China and Russian Turkestan (Central Asia). The latter exhibit the combination of a rather strong F with a rather weak M. 3. *Weak races.* These occupy all Europe. Races have been tested from Central Europe, the Alps, Italy, Spain, The Balkans, European Russia, all being weak. But in addition to these a weak race occupies the northernmost Island of Japan, Hokkaido—as a matter of fact, the weakest of all known races. Among these weak races we have to distinguish three types. 1.) a. The race Hokkaido. This race, the very weakest, is characterized by the following combinations: Hokkaido ♀ × strong ♂ produce only males, half of which are sex-reversal-males i.e., the final type of female intersexuality. b. The combination of an F of a strong race with two M from Hokkaido, F_{Str} M_{Hok} M_{Hok}, is a sex-reversal-female (for details see later). 2.) The weak races s.str. These are characterized as follows: a weak ♀ × strong ♂ again produces only sex-reversal—♂. The combination of a strong F with two weak M, F_{Str} M_w M_w, however, is an intersexual male. Of these weak races again different degrees could be distinguished by special tests (see later). No regularity regarding their distribution in Europe could be found. 3.) The half-weak races. These occupy a position between the neutral and the weak races. A half weak ♀ crossed to a strong ♂ produces in F_1 all female intersexes (no-sex-reversal-♂), the typical degree of intersexuality,—which, however, differs in different crosses being dependent upon the relative half-weakness of the mother's race and the relative strength of the father's race. The combination of a strong with two half-weak M is an intersexual ♂ of lower intersexuality than before (No. 2). Half-weak races with absolutely irregular distribution have been found in one valley of the European Alps (Tessin). They occur also in Japan in the Island of Kyushiu at the south-western end of the neutral races and in Korea. The gipsy-moth introduced from France into New England also belonged to this type as tests made 15 years ago showed.

The description of these races shows already that by proper combination of the weak F with the strong M all degrees of female intersexuality from the first beginning to complete sex-reversal can be and have been produced in an orderly manner in 100 per cent of the would-be daughters.

Whenever by such a cross the relative weakness and strength of the F and M genes of two races has been tested, the results of crossing the same race of females with males of different races or different females with the same males may be predicted. The results of innumerable crosses always came out according to expectation. Whenever a cross for female intersexuality gave information about the type of the two races involved, the expectations for

all other hybrid combinations, especially those for producing male intersexuality, could be predicted and never a failure occurred, as the immense experimental material in our publications proves (see No. 20, 25, 28, 40, 42).

In these different crosses and still more in the complex crosses later to be discussed we had a chance to find out whether the different degrees of strength and weakness of F and M respectively were properties of the same gene or caused by additional (modifying) genes. The results always were such as would be expected if the same genes F and M were present in the different races in different condition, called here strength or weakness. In other words the different F and M of the genetically different races proved to be a series of multiple allelomorphs of F and M. The result of a given combination in regard to sex is therefore determined by the balance or unbalance between the relative strength of the present members of the F series and the M series. To give a few concrete examples:

$F_{Soeul}\ M_{Gifu}$ = low grade ♀ J
$F_{Soeul}\ M_{Tokyo}$ = high grade ♀ J
$F_{Tessin}\ M_{Tokyo}$ = very high grade ♀ J
$F_{Kumamoto}\ M_{Tokyo}$ = very low grade ♀ J
$F_{Hokkaido}\ M_{Tokyo}$ = sex-reversal ♂
$F_{Kyoto}\ M_{Tokyo}$ = normal ♀
$F_{Tokyo}\ M_{Kyoto}\ M_{Kyoto}$ = normal ♂
$F_{Tokyo}\ M_{Hokkaido}\ M_{Hokkaido}$ = sex-reversal ♀
$F_{Tokyo}\ M_{Soeul}\ M_{Soeul}$ = very low inters. ♂
$F_{Tokyo}\ M_{Berlin}\ M_{Berlin}$ = medium inters. ♂
$F_{Tokyo}\ M_{Trentino}\ M_{Trentino}$ = very high inters. ♂

These results finally show that the different conditions of the F and M of the different races, called their weakness or strength, form an orderly, quantitative series in regard to their effect and that the different possible combinations behave exactly as if the different degrees of strength of these genes could be expressed in numerical values, which, if substituted for each other, change the effect with a corresponding measure. This led me first to identify the relative strength or valency of these genes with their absolute quantity (17), measured in numbers of molecules. This identification will be discussed later, because the whole analysis is exactly the same, whether we speak concretely of the quantity of the genes or agnostically of their strength or valency.

TWO METHODS OF TESTING THE WHOLE CONCEPTION

The correctness of the analysis has been further tested by two methods, which furnish most decisive proofs:

a. The substitution test for gametic males (see 20, 25, 28, 40, 41, 42). It has been mentioned that XX-Individuals of the genetic constitution $F_{strong}\ M_{Hok}\ M_{Hok}$ i.e. would-be males with the F of a strong race and two M of the weakest race, Hokkaido, are sex-reversal-♀, because the two M are so weak that the strong F outbalances them completely. The back cross (Tokyo ♀ × Hokkaido ♂) × Hokkaido ♂ therefore produces nothing but females, half normal, half sex-reversal. Of these most of the sex-reversal-♀ die. (See later.) By backcrossing the same F_1 ♀ (Tok × Hok) with a third known race we replace in the resulting would-be sons one of the M of the forementioned combination by the M of this third race. If this be the race A this substitution-experiment runs the following way: F_1 ♀ (Tok × Hok) $[F_{Tok}\ M_{Hok}]$ × ♂ race A $(F_A M_A M_A)$ = ♀ $F_{Tok}\ M_A$ + ♂ $F_{Tok}\ M_{Hok}\ M_A$. The females are all normal because the maternal line (the F) which comes from Tokyo, is strong. What are the males? It is evident that in case our analysis is correct, the result depends entirely upon the strength of our Race A. If M_A is a little stronger than M_{Hok} the MM individuals must be high grade intersexual males instead of sex-

reversal females. If we substitute now an M from a race still a little stronger the resulting MM individuals must be males of lower intersexuality than before. Continuing this substitution with M of other races of increasing strength intersexuality of the resulting sons must decrease until with a given strength of the substituted M only normal sons are produced. The results of such a substitution experiment must be in harmony with all other experiments from which an evaluation of the respective strength of the different races is derived. Among the many experiments of this type which have been carried out again and again every single one gave the expected result.

These experiments can and have been varied in different ways, each one being a check on the other. For example, we may start from the combination F_{Tok} M_{Hok} M_{Hok} and leave the two M_{Hok} but substitute another F from a different race, say F_A. Then with decreasing strength of the substituted F the degree of intersexuality of the MM individuals must also decrease. A parallel series may be built up with other M, for example M_{Hok} M_A or M_AM_A or M_AM_B. Again a parallel shifting of the degree of intersexuality is to be expected. A large number of such experiments gave invariably the expected, consistent results.

It is evident that it would be possible theoretically to calculate from such a series relative values for the strength of F and M of the different races. In fact this cannot be done. There is a certain amount of fluctuation among the MM individuals which is measured by a classification which cannot be made mathematically exact. At both ends of the series the fluctuation transgresses into the normal (low grade intersexes into normal maleness, high grade intersexes into normal femaleness). But there is no possibility of distinguishing more or less male males or more or less female females. These facts prevent a calculation of sufficient exactitude.

b. The tests by complex crosses. These tests control not only the relative strength of the different F and M but simultaneously test most decisively the maternal inheritance of F and the situation of M within the X-chromosomes. In these experiments we make crosses involving more than two different races. There is no limit to the complexity of these crosses or the races involved. If sex is controlled only by the relation of F:M, if further these sex-genes are inherited as indicated and if the relative strength of the races involved is known from previous experiments the result of any combination can be predicted. Let us take an example. Let A, B, C, D, E, G be six different races. We cross A ♀ × B ♂, breed from this F_2, written $(A \times B)^2$; cross an F_2 ♀ to a male, which is an F_1 hybrid between C and D, that is $(A \times B)^2 \times (C \times D)$; a daughter from this combination is crossed to a male which is an F_2 hybrid from A × E, i.e.

$$\left[(A \times B)^2 \times (C \times D)\right] \times (A \times E)^2$$

and finally cross a daughter from this combination to a pure male of the race G. The resulting complex cross is then

$$\left[\left[(A \times B)^2 \times (C \times D)\right] \times (A \times E)^2\right] \times G$$

If our analysis is correct, the resulting daughters are expected to be the same as if only the cross A ♀ × G ♂ had been made. The maternal line in which the gene F is inherited is derived from the race A, all offspring of this complex cross therefore must have the F_A. The X-Chromosome of a daughter is derived from her father, therefore all daughters of this cross must be F_AM_G. If A is a weak race and G a strong one, all these

daughters are intersexual of the same type as the simple cross A × G. Generally speaking, the daughters must be what they are in the simple cross A × G, according to the relative strength of these two races. A very large number of such crosses in the most diversified combinations have been made and invariably they came out according to expectation. As a matter of fact, we have used these complex combinations for many years to keep lines of a definite F, in cases where the pure race is sensitive to inbreeding and dies out after a few generations. The respective gene F of these races is kept in stock by making any crosses whatsoever in the maternal line of the race, provided they also produce normal daughters.

What are the expectations for the sons of such a complex cross? If the maternal line—the first symbol in a formula of the type as above—is weak, only normal males are expected and are in fact bred. If, however, the maternal line belongs to a strong race intersexual males may appear if the proper FMM combinations are possible. In this case an extremely decisive test can be made, as will be shown by an actual example. The following complex cross has been built up among many similar ones:

$$\left[(\text{Ao} \times \text{Hok})^2 \times \text{Span}\right] ♀ \times \left[(\text{Ao} \times \text{Hok})^2 \times \left[\left[(\text{Tess} \times \text{Hok}) \times (\text{Kum} \times \text{Gif})\right] \times \left[(\text{Tess} \times \text{Hok}) \times \text{Ao}\right]\right]\right] ♂$$

Ao is the very strong race from Aomori; Hok, the very weak race from Hokkaido; Span, the very weak race from Spain; Tess, the half weak race from the Tessin valley (Switzerland); Kum, the neutral race from Kumamoto; and Gif, another strong race from Gifu, different in strength from Ao. Thus in this combination six different races representing as many different grades of strength are combined in the complex way indicated by the formula. Seventy-seven sisters of the three-race combination in the left bracket were mated to 77 brothers of the five race combination in the right bracket. The maternal line (the great grandmother of these sisters) belongs to the very strong race Aomori and therefore all offspring must have the strong F_{Ao}, which permits only normal daughters but makes possible the production of intersexual males. According to expectation the result regarding the males depends entirely upon the F and the two M, i.e., the two X-chromosomes, one of which is derived from the mother and one from the father. The sisters used in this cross received their own X from the Spanish father and therefore all sons of the complex cross must have the constitution F_{Ao} M_{Span} M_X. What are the expectations for the second M_X? From the racial constitution of the five race fathers used in our cross, it can easily be derived that theoretically these males might have the following 16 combinations of their two M (X).

1. Ao Hok	9. Hok Hok
2. Ao Kum	10. Hok Kum
3. Ao Ao	11. Hok Ao
4. Ao Kum	12. Hok Kum
5. Ao Hok	13. Hok Hok
6. Ao Gif	14. Hok Gif
7. Ao Ao	15. Hok Ao
8. Ao Gif	16. Hok Gif

But whereas these males were all brothers, i.e., sons of one father and one mother, their parents and grandparents of course also were always a single pair [no mass cultures], and whereas from a single mating of a female M_K with a male M_AM_A or M_AM_B or M_BM_C never more than two MM combinations can be derived, the result of our cross of the 77

sisters to 77 unrelated brothers can be at most only the production of two types of sons. Which type appears, depends upon the unknown constitution of the fathers, mothers, grandfathers, etc. of these males in regard to their X-chromosomes. But whatever the result, a simple analysis shows that the two types which might appear cannot be any two combinations of the 16 possible male types with the maternal F_{Ao} M_{Span} but only two definite ones belonging together, namely, those produced by males 1 and 2, 3 and 4, 5 and 6 etc. respectively. The 16 types of males then might produce with the M_{Span} of the mothers the following 16 XX or MM combinations among the sons of our complex cross. The phenotypical appearance of these male combinations follows from our previous knowledge of the relative strength of the races. And within one fraternity as in our experiment, only two combinations are allowed to appear and in addition only two belonging together namely, 1 and 2, 3 and 4, etc. The following are the possibilities:

	MM (XX) COMBINATIONS	EXPECTATION FOR ENTIRE OFFSPRING
1	½ Span Ao ½ Span Hok	3 ♀ : 1 ♂*
2	½ Span Ao ½ Span Kum	2 ♀ : 1 + ♂ : 1 − ♂ ♪
3	½ Span Ao ½ Span Ao	normal sexes
4	½ Span Ao ½ Span Kum	like Nr. 2
5	½ Span Ao ½ Span Hok	3 ♀ : 1 ♂*
6	½ Span Ao ½ Span Gif	normal sexes
7	½ Span Ao ½ Span Ao	normal sexes
8	½ Span Ao ½ Span Gif	normal sexes
9	½ Span Hok ½ Span Hok	only ♀
10	½ Span Hok ½ Span Kum	3 ♀ : 1 ♂ + ♂ ♪*
11	½ Span Hok ½ Span Ao	3 ♀ : 1 ♂*
12	½ Span Hok ½ Span Kum	like Nr. 10
13	½ Span Hok ½ Span Hok	only ♀
14	½ Span Hok ½ Span Gif	3 ♀ : 1 ♂*
15	½ Span Hok ½ Span Ao	3 ♀ : 1 ♂*
16	½ Span Hok ½ Span Gif	3 ♀ : 1 ♂*

* In fact 2 ♀ : 1 ♂ on account of the inviability of the sex-reversal-females, see later.

I recall that F_{Ao} M_{Hok} M_{Span} and F_{Ao} M_{Hok} M_{Hok} are sex-reversal = ♀♀, F_{Ao} M_{Span} M_{Kum} male intersexes with fluctuation into normal males, and F_{Ao} M_{Span} $M_{Ao\ or\ Gif}$ normal males. The actual result was: 39 cultures with normal sexes and 38 cultures with females, males and intersexual males of the F_{Ao} M_{Span} M_{Kum} type i.e. combinations 3 and 4, altogether about 3400 individuals. I dare any of the critics of our general views, who indulge in general utterances of liking or disliking or possibly modifying factors without trying to analyze the experimental results, to explain this and many other experiments of this type in concrete genetic terms, different from our own.

SEX-REVERSAL MALES AND FEMALES

It is desirable to offer proof that the sex-reversal ♀ ♀ and ♂♂ which form the ends of the two intersexual series have the gametic constitution of their original sex.

1. *Sex-reversal-♂♂* (see 28). A considerable percentage of sex-reversal-♂♂ have been found fertile so that their genetic constitution expected to be XY can be tested. As they cannot be distinguished from ordinary males (for exceptions see the original papers) the only procedure is to mate every single son of a cross weak ♀ × strong ♂, producing only sons, to a female of known constitution. Of the many tests carried out with positive results the following is the most conclusive: We use the females of an F_1 combination Tokyo ♀ × Berlin ♂. Their genetic constitution is F_{Tok} M_{Berl}, Tokyo being a strong race and Berlin a weak one. These females we cross to all brothers from the combination Hokkaido ♀ × Tokyo ♂, giving only males in F_1. If there are sex-reversal-males of XY—constitution among these, our test crosses must consist of two types:

1. F_{Tok} M_{Berl} ♀ × genuine ♂ M_{Hok} M
2. F_{Tok} M_{Berl} ♀ × reversal ♂ M_{To}

The first combination gives (disregarding the always normal ♀ in the strong maternal line): ½ ♂ F_{Tok} M_{Berl} M_{Hok}, ½ ♂ F_{Tok} M_{Berl} M_{Tok}. We know from former experiments that a male F_{Tok} M_{Berl} M_{Tok} is a normal male but that an individual F_{Tok} M_{Berl} M_{Hok} is a high-grade intersexual male. The combination 1 therefore must yield normal females, normal ♂ and high-grade intersexual ♂ (of these less than half on account of their inviability). The cross No. 2 however produces only males F_{Tok} M_{Berl} M_{Tok} i.e., normal ♂ besides YY individuals which are expected to be non-viable. The expectation is here therefore 2♀ : 1♂ and no chance whatsoever for intersexual males. The actual result of such a combination was, when 48 males from an only-male-cross were tested: 29 cultures gave ♀, ♂ and high grade intersexes of the known type, 19 cultures gave only normal sexes, the ratio being 1.8 ± 0.052 ♀ : 1 ♂. This experiment and others of a similar type prove the XY condition of the sex-reversal-males.

2. *The sex-reversal-females* (see 31, 32, 35, 40). Unfortunately the situation for these is different. Hundreds of females of all female cultures, which have been tested in proper experiments, proved to be normal XY females. It was, however, shown that contrary to the behavior of intersexual females, which have a rather high degree of viability, the viability of intersexual males decreases rapidly with increasing intersexuality, the highest types being viable only in a very small percentage. This is still more the case with sex-reversal females, only three having been found altogether, which could be recognized morphologically but which were sterile. Under these circumstances a test for sex-reversal-females corresponding to the one for sex-reversal-males could not be carried out. A probable explanation for the different behavior of sex-reversal ♀ and ♂ is furnished if the inheritance of F within the Y-chromosome is taken into account.

MODIFYING GENES

It has been mentioned before that in F_2 of the type (strong ♀ × weak ♂)^2 one-half intersexual males are expected, but that this ratio is not always found. Most frequently only about 1/8 intersexual males appear. In analyzing these cases it was found that in strong Japanese versus weak European races, besides the differences in regard to F and M, a pair of autosomal allelomorphic modifying factors is involved, which affects the expression of male intersexuality (see 20, 40, 53, 56–58). These allelomorphs Tt recombine in F_2 with the MM in simple Mendelian way, thus showing their situation within an autosome. The individuals M_{weak} M_{weak}, i.e. half of the F_2 males, ought to be intersexual, but the dominant gene TT prevents this, thus leaving only the tt combinations (1/4 of the M_{weak} M_{weak} males and therefore 1/8 of all males) to become intersexual. [In a certain race combination, which was studied by Lenz, even two such modifying genes were probably present.] The expectations for further generations of such crosses, especially the isolation of pure tt combinations with M_{weak} M_{weak} giving 100 per cent intersexual males, were fulfilled (56–58). Though every detail concerning the action of these modifiers in different combinations is not yet clear, the general facts, as reported, are proven. A parallel case in *Drosophila* is known from Sturtevant's work.

It might be useful to discuss briefly the existence of such modifiers, because misconceptions in regard to the meaning of modifying factors are widespread in genetic literature and lead to still

worse conceptions regarding sex-determination. The action of every gene is normally matched and in tune with the whole genetic system of the developing organism. Otherwise no typical result would be possible. Each mutation in one gene may upset this whole system, provided that the mutated gene acts on a more or less early developmental process. In such a case, therefore, any mutation of a gene might interfere with the action of any other gene. In other words, any mutated gene might act as a modifier to any other gene in action. If we express this more physiologically we may say that a mutated gene might change the internal environment so that the effect of any other gene is consequently modified. Physiologically, of course not genetically, a modification of the internal environment (whatever this may mean in a definite case) brought about by the action of external stimuli is identical with a corresponding change brought about by a mutated gene. It is exactly such a situation which we meet in regard to sexuality. Sexuality (viz. intersexuality) may be shifted by action of external influences within normal genetic constitution (see later). Sexuality or intersexuality may also be shifted by the action of a modifying gene. We may sooner or later find any number of such modifiers, acting in the female as well as in the male direction, of which as many might exist as mutating genes occur. Superficial reasoning will lead then to the conclusion that sex here is determined not by the $\frac{F}{M}$ balance but by the action of these modifiers pulling towards femaleness and maleness respectively. If such a conclusion were possible, the same reasoning would have to be applied to the action of every gene. In other words, there would be no possibility of connecting any gene with a definite effect. This connection, however, is unavoidable in genetic analysis, though the unspoken addition is to be kept in mind that such a connection applies only to a definite genetic system (containing all the other genes and the specific plasmatic basis) and is changed with almost each gene-mutation or with change of external conditions capable of influencing the system in which the gene acts. Obviously, therefore, the discovery of modifying genes influencing sexual expression has nothing to do with the primary sex-determining agency, the $\frac{F}{M}$ balance, demonstrated conclusively in our experiments.

THE MATERNAL INHERITANCE OF F

The maternal inheritance of F in the gipsy moth is a hundred-fold tested experimental fact (17, 20, 25, 28, 40, 42, 53). Cytologically it might mean that F is either inherited in the protoplasm or in the Y-chromosome. As early as 1912 I pointed out the possibility of F being situated within the Y-chromosome. When the maternal inheritance of F was later established I was constantly looking for a chance to decide this point. This possibility seemed to be given in a genetical analysis of the exceptional females which occasionally occur in all-male combinations. Of all explanations of their origin only one fitted the facts, namely that they are caused by non-disjunction of the sex-chromosomes. If this was the case certain genetical consequences had to follow and all tests which could be carried out agreed with the expectations (20, 27). It was, therefore, concluded that F is most probably inherited within the Y-chromosome; this being, by the way the first case in which the situation of a definite gene

within the Y-chromosome could be made probable by genetic tests. However I never claimed more than probability for this case, first because an absolutely decisive proof could not be obtained and second because one of the consequences was rather shocking, namely that F in the male must have finished its action already before maturation-division, which eliminates the Y in the male eggs.

We are now confident that this final proof will soon be forthcoming. We have found a new genetic test of great value, which also makes possible a cytological control. Experiments are under way. Another chance is given by a recent discovery of Kosminsky (52). He found within normal weak races in Russia lines which regularly throw male intersexes, just as if the F were a strong F. I have suggested that his interesting experiments might be explained by a certain abnormality of the X-Y mechanism. If further experiments prove the correctness of my suggestion a decisive and final test for the inheritance of F within the Y-chromosome will be furnished. We are confident that within the next few years this point will be finally settled.

MORPHOLOGICAL ANALYSIS OF INTERSEXUALITY

At first sight intersexes appear macroscopically as definite stages between the sexes. Closer inspection reveals that the respective stage is the consequence of the amount of admixture of structures of the other sex with the gametic sex. Different organs, however, behave differently according to some law. This was found when it was realized that the intersexual transformation of each organ is reciprocal to the order of differentiation of the organs in development. This fact led to the conclusion that an intersex is developed up to a certain point with the gametic sex (XX or XY), and from this point on, the turning point, differentiation occurs under the control of the other sex. The degree of intersexuality is, therefore, a direct function of the situation of the turning point in development. This conclusion had to be tested in two ways. First it had to be shown that the increasing series of intersexuality consists for each organ in the addition of a series of developmental stages of the organ with the other sex to the basis of the organ with the original sex. Extended morphological studies for each organ with sexual differentiation made by me and my collaborators (Poppelbaum, Minami, Saguchi, Du Bois) have proven this point. Some additional facts have been discovered by Kosminsky. As a description of all morphological details would require too great space and much illustration, we refer the reader to the original publications, where some of the difficulties in the details are also discussed (17, 20, 23, 25 26, 29, 31, 35, 46, 50, 51, 54, 55). Though not every single point is completely cleared, the positive evidence is overwhelming. As a matter of fact the analysis is in perfect agreement with the result of the investigation of other cases of intersexuality; viz. the case of *Bonellia*, as studied by Baltzer previous to our analysis, the case of triploid intersexuality in moths, as analyzed by myself (30, 37, 44), and recently the case of triploid intersexuality in *Drosophila* studied by Bridges and Dobzhansky (8).

The second method of attack is the study of the development of different organs in intersexes. An absolutely positive answer has been found in our study of the sex-reversal of ovary and testes in the development of intersexual females and males, where every stage of the transformation of an ovary into a testis and vice versa has been followed (35, 46). Not yet com-

pletely clear are the facts regarding the antennae, where some points of difference in detail exist between Kosminsky and myself (25, 28, 50). Positive again are the results of my student Miss Du Bois regarding the complicated copulatory organ, and Miss Du Bois is now busy filling the last gaps. Altogether the morphological and embryological explanation of intersexuality is safely established.

CONNECTION BETWEEN GENETIC AND MORPHOLOGICAL ANALYSIS

If we put together the genetic and the morphological results, the following situation is safely established: The genetic basis of sex and intersexuality is given by the amount of balance or unbalance between F and M at the beginning of development. The effect of this genic situation is that at a certain moment in development the turning point occurs, that the control of sexual differentiation is shifted from the F to the M genes or vice versa. And the time of occurrence of this event is a simple function of the relative degree of balance or unbalance of $\frac{F}{M}$. These three sets of facts in our opinion cannot be coördinated except under the view that F and M respectively are responsible for a sex-determining or sex-controlling reaction, both of which proceed with a definite velocity proportional to the "strength" or "valency" or, as we see it, quantity, of these genes; that the quicker reaction controls the alternative of sexual differentiation; and that the two curves of the F and M reactions may have points of intersection at a given moment, the turning point, if the quantities of F and M are not properly matched. This inevitable conclusion we have represented in the form of graphs, which are amply discussed in our papers (since No. 17, see 28) and which have been already discussed in this journal in Crew's articles on sex. We refrain, therefore, from further details.

This physiological solution of the whole problem could be tested by trying to change the relative velocities of these two reactions within a pure normal race by the differential action of temperature. An experiment of this type was already performed in 1909 by Kosminsky, (47) before anything was known of intersexuality. This author as well as myself have since extended these experiments with positive results, producing intersexuality by action of extreme temperature within a pure race (9, 24, 28, 48, 49). Further work on these lines is in progress.

THE X-CHROMOSOME MECHANISM

The analysis of intersexuality has furnished a simple explanation for the two X- one X-mechanism of sex-chromosomes (see 21). This mechanism proves to be an ideal method of securing that of two simultaneous and competing reactions, the F and M reactions, one or the other obtains the higher speed and therewith control of differentiation. The mechanism acts by matching the constant F reaction (maternally inherited F in the gipsy moth, homozygous FF otherwise, M instead of F in the Drosophila type) with one produced either by one or by two M (F in the Drosophila type), that is, in case of velocities proportional to the quantities, with a slower or speedier M-reaction.

VALENCY OR QUANTITY OF SEX-GENES

We have identified the different valencies or strength of the sex-genes, which are experimental facts, whatever term of description we choose, with their absolute quantities, a conclusion to which many geneticists are vehemently opposed, though no one has tried to make a better

suggestion (vague talk about stereoisomery or polymerisation does not lead to any improvement). It cannot be denied, that a mathematical proof for our conclusion is impossible, so long as a gene cannot be weighed or the numbers of its molecules calculated and conclusions can only be based on definite orderly behavior in regard to the effect of the gene. Agnosticism, is, therefore, an irreproachable attitude. Some people, however, are not content with agnosticism and cannot help trying to formulate theories. The value of such theories is not to be judged by declarations ex cathedra like "neither sound genetics nor sound physiology" but by the recognized demands on any theory in science. In the present case we believe we have shown that we cannot get away from a theory of a quantitative type, that other possible theories entail more difficulties and that our theory, though not the only possible one, is the one which accounts most simply for the thoroughly orderly and consistent series of experimental facts. I am of course the first to accept a better theory, but I am not ready to acknowledge the value of vague and non-commital phrases. The whole question has been recently discussed in No. 42.

THE THEORY OF PLUS-MINUS-MODIFIERS

Though no other explanation for the results of our work has thus far been presented, a modification of our concept has been used by *Bridges* (4–7) in his analysis of triploid intersexuality. There can be no doubt that our analysis of diploid intersexuality can as well be applied to the facts of triploid intersexuality. As a matter of fact, the discoverer of triploid intersexuality, Standfuss, has already done this (60, 61) and we have repeatedly shown the same (30, 33, 37). But how about applying the conceptions of Bridges to the case of diploid intersexuality, which must be possible if that conception has any merits? Bridges agrees with us that genetically sex and intersexuality are controlled by a balance or unbalance between female and male determiners. But as in triploids this balance finds its visible expression in the relative numbers of autosomes and X-chromosomes and as definite genes could be located in the autosomes, which influence sexual differentiation (modifiers), he preferred to replace the notion of $\frac{F}{M}$ of definite valency or quantity by a number of plus or minus modifiers, pulling sex towards the female or male side. We have exposed already the error which is made, if from the genetic proof of modifiers for sex a conclusion is drawn on the primary mode of sex-determination. In the triploid Drosophila-case it is proven that the X-chromosomes contain F genes (Drosophila type of male heterogamecy) without any possibility of claiming more than one such gene if one keeps within the experimental evidence. It is further proven that the M effect is produced by genes in the autosomes, which increase their effect by doubling their number, nothing however being known as to whether they constitute a single pair of allelomorphs or not; and further it is known, that modifying genes exist, probably for pulling sex in either direction. To dissolve the F and M genes into numbers of plus and minus modifiers finds no basis in the experimental evidence, except when basing the conclusions on the forementioned error. But as there can be no doubt any more after the morphological work of Dobzhansky and Bridges in *Drosophila*, following our analysis of triploid intersexuality in moths, that the phenomenon of intersexuality is identical in diploid and triploid intersexuality, the plus and minus modifier idea ought to

work as well in our case and eventually form an improvement over our analysis. If we try such an application, it is at once clear that in diploid intersexuality, where everything occurs within the normal chromosome set and where the genetic behavior of the things called F and M is completely known, the plus and minus modifier idea can only mean that what appears in heredity as single genes F and M is in fact a completely linked group of plus and minus modifiers; further that what we called the different strength or quantity of these genes is in fact the combined strength or additive effect of different numbers of these modifiers. In other words, everything is as it was, except for replacing a definite number of gene-molecules by a definite number of completely linked genes. What advantage can be gained by such procedure, but to save genetics from such a heresy as the idea that a gene is composed of a definite number of molecules? I cannot help therefore regarding the plus-minus modifier idea as a minus modification.

GENERALISATION

The general theory of sex-determination derived from the analysis of intersexuality in the gipsy-moth has been applied by me and many biologists who accepted my views to the animal and plant kingdoms generally, including also types where the zygotic sex-determination is obscured by the addition of the action of sex-hormones (see 11, 15, 16, 18, 21, 34, 36, 37, 38, 39). These generalisations, which seem to me convincing, in spite of many details waiting for further analysis, and which are being accepted by a steadily increasing number of biologists, are found in a larger set of publications, which have been partly reviewed already by Crew in this journal and have inspired the discussion in a number of modern books and reviews of the problem of sex, l.c., by Crew, Bonnier, Hartmann, Weissenberg, Witschi, etc.

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BIOLOGICAL ORGANIZATION

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"Ni les recherches spéciales, ni les vues générales, ne suffisent isolément à constituer aucune science: c'est par leur alliance, par leur union, qu'elle se fonde et se développe."—CLAUDE BERNARD.

PHYSICAL concepts have no necessary finality or inevitability. They take rise in response to pragmatic requirements and in harmony with the context of ideas and phenomena prevailing at the time of their birth; they persist in proportion to their success in the symbolic formulation of these and of new experiences. The recent revisal of the foundations of physics required by relativity and the new quantum theory has shown us clearly that as this context changes the concepts themselves change their significance and indeed may lose their original meaning. The physicist is thus faced continuously with the duty of sceptically reviewing his trusted theories and of discounting as far as possible those features of them which can be traced to historical accident. Further, as these new advances have taken him close to the fields of philosophy, psychology, and biology he is bound to recognize that the experiences of these sciences must be understood in each new formulation of his own concepts.

Physics abstracts certain limited features from the richness of direct experience, but no further attention is paid to the other aspects of the total experience from that point on. The assumption is made that these other aspects can be ignored always because they can be ignored in the initial stages. As our knowledge and insight grow, we may find that the old concepts and symbols are not best adapted to apprehending and summarizing newer phenomena. From time to time aspects of a problem arise which require that we take into consideration unfamiliar kinds of abstraction. Any set of abstractions by its very nature places a fatal limitation on the kinds of observations we will make or on the range of problems which it can handle successfully. At the time of Newton, it was not quite clear that the characteristics of matter and space so far discovered (such as extension) were sufficient to account for everything (31). The distortion implicit in physical concepts because of certain vaguenesses about methods of measurement has been demonstrated and corrected by Einstein and Heisenberg (1) and analyzed in detail by Bridgman (3), but other viewpoints of the intuitive Newtonian picture of the world are just as much in need of a constant vigilance and revision. Such considerations have in the past been avoided because they are metaphysical.

The suggestions of the following paper may have value in directing thought in biology and psychology along somewhat new lines. It is true that much thinking in the latter sciences has divorced itself from the bounds imposed by the physicists' view of the world, but there is a large region in both sciences where the rigor of thought and resource in experi-

ment characteristic of physics form the dominant note. The biologists and psychologists engaged in exact investigation are still operating in a mood of exaggerated awe of the methods, concepts, and laws of physics, and they will not be inclined to side with the philosophic followers of their own sciences if it cannot be shown to them that physics itself will countenance a more tolerant point of view. Hence the necessity of indicating where some of the unsolved problems of biology impinge on physical theory and of endeavoring to modify the physical picture in the light of them. This process reacts favorably on physics itself, for it brings to light some inadequacies of theoretical physics. In particular, I shall attempt to show that the second law of thermodynamics as ordinarily understood does not tell us as much about organic systems as we thought it did.

As an illustration of such an unsolved problem, one might consider the growth curve. In a recent paper, J. Gray (11) concludes that "although it is possible to express the final growth curve by means of a simple formula, this formula cannot throw any real light on the underlying causes of growth." His paper is taken up largely, however, with a discussion of the best differential equation for the curve. This dilemma is typical of those produced by the quantitative, atomistic, differential-calculus, Euclidean ideology of many contemporary biologists and can only be corrected by a more qualitative, configurational or organismal, logico-symbolical, phenomenological viewpoint.

THE MEANING OF PROBABILITY

The concept of probability needs an examination from the above point of view. The arbitrariness inherent in it has been brought to attention recently by the establishment of new statistical theories in which the calculation of the probability of a state proceeds on different bases from that employed with such success in the classical kinetic theory of gases and in statistical mechanics. The disconcerting nature of probability as a physical concept had been recognized before this development by mathematicians, physicists, and philosophers. If we are to calculate the probability of an event, it is certain that our estimate will depend on the relevant facts at our disposal. If we know all the circumstances which can affect the occurrence of this state or event, the probability can have only one of two possible values: 1 or 0. (This is the case in classical deterministic mechanics.) Intermediate values are possible only because of our ignorance of one or more of the controlling factors.

This is the state of affairs with thermodynamic quantities. "If the atomic picture could be made more definite, the meanings which we. . . . attach to entropy and temperature would be dissolved. Our theory of these entities depends upon the vagueness of the picture." (Darrow, 6). The same indefiniteness is to be found in the new quantum theory. "The new mechanics knows no exact initial conditions. The classical alternative—either necessary or impossible—it does not know. For it, *anything* is to be regarded as possible. Correspondingly, the new mechanics does not concern itself with the discovery of certain events having a given probability, but with the study of *the probability of all possible events*." (Condon, 5). The latter admission is of intense interest for the Gestalt considerations of the last section of this paper.

From this point of view, the accepted value of the probability in a given case reflects the state of our knowledge regarding the phenomenon. As Eddington (9)

has said, "Unless it belies its name, probability can be modified in ways which ordinary physical entities would not admit of. There can be no unique probability attached to any event or behavior; we can only speak of 'probability in the light of certain given information,' and the probability alters according to the extent of the information." Further, probability depends on the *assumptions* made or *conventions* used in calculating it, even though the "facts" are agreed on.

Briefly, probability depends on: (1) the facts known, (2) the assumptions used. If I toss a coin ten times and get ten heads, the probability of throwing a head on the next trial depends on the assumptions I am willing to make as to the importance of the knowledge derived from the previous trials, it being assumed that I have no other knowledge about the coin. Now if I were tossing the coin for the first time, I should take the probability of head to be one-half, which I should also conclude in this case for the eleventh trial if I act on the assumption that a biassed or false coin is essentially impossible. However, if I admit the assumption that the coin may be biassed or double-headed, or that the method of throwing may be selective, then the ten successive heads give me a measure of the probability that the coin is defective (Bayes' formula), and the probability of head on the next throw is now greater than one-half. After one hundred throws of head, I should, on the second assumption, be still more certain that the coin was defective and so the probability of a head on the one hundred and first throw would be nearly 1. Thus our estimate of the probability in this case depends on the facts known (the number of previous successive heads) and on the assumptions (allowability of defect, etc.). This aspect of probability is discussed by C. J. Fry (10): "To the Omniscient the answer (of a probability problem) is either unity or zero. It is neither to us, because we are in neither state of knowledge (omniscient or completely ignorant). . . . Probability is a measure of the importance of our state of ignorance, (and) it must change its value whenever we add new knowledge." Or, A. J. Lotka (22): "Probability is essentially a matter of classification. An improbable event is one that is a member of a small class and whether it is so or not depends on our system of classification". . . . "It is meaningless unless the characteristic with regard to which the probability is reckoned is explicitly or implicitly indicated." In the same connection, Reiser (27) decides: "In this view, probable knowledge does not imply contingency or indetermination, but only that our knowledge is insufficient to predict the event. This interpretation seems to render 'chance' subjective."

It is possible that while the conventional classification is most valuable for physics there may be other methods for living systems—which depend for their success on the more immediate accessibility (in organic systems) of other kinds of facts. The suggestions of this paper are based on such a possibility. Other classifications of the possible arrangements, however, are subject to grave difficulties because they do not have the advantages of (1) homogeneity or uniformity throughout all nature, (i.e., they cannot be applied in the same way to all systems: they must be specific or so to say, local); or of (2) quantitative manipulability. The abstractions on which these other classifications are based are qualitative or non-commutative: their most important criterion is morphological or functional. Our common everyday mathematics does not lend itself to the treatment of these. Fortunately, A. J.

Lotka has suggested ways of attacking the problem, and J. H. Woodger has shown the way to the application to it of the method of symbolic logic and related fields. In the words of the latter (39): "It is not so much the mathematical laws of physics that we require but the mathematical *method* in the broadest sense, and this is quite a different thing." (Different from "applying" mathematics to biological data.) We need a "revision of our ways of thinking," and shall have, for this purpose, to explore some little known regions of the "realm of abstract logical entities and relations."

ENTROPY

The entropy concept is related to the probability of a state by the Boltzmann formula:

$$S \text{ (entropy)} = k \log \text{ (probability)},$$

where k is the gas constant for a single molecule, and we must conclude that the factors which influence the probability—in particular the knowledge or assumptions about the phenomena—will also change the value of the entropy.

The tendency towards increased entropy which we call the second law of thermodynamics was reread by Boltzmann to mean that *a system of itself will change from less probable to more probable states*. This statement gives us at first an agreeable feeling of increased understanding but it is "indefinite in failing to specify with regard to what characteristic probability is to be reckoned." (Lotka, p. 35.) A. J. Lotka has given a very convincing example to show that the apparent improbability of a *rare* event is due to our *method of regarding* the event. A series of 50 cards is numbered 1 to 50 and equally divided between two urns, A and B. A card from A and one from B are interchanged and their numbers noted, this process being repeated several times. After any arbitrary number of exchanges, the cards in urn A are painted black on the reverse side and the interchanging continued as before. From this point on, the behavior of the system is conventional; i.e., it changes continuously towards the ("most probable") state of equal distribution of black and white between the two urns (12½ in each). Before the painting, however, the change is towards the "very improbable" distribution in which all the white cards are in one urn and all the black in another. In other words the "probability" characteristics of a system are imposed on it by ourselves for convenience. A "rare" event is one which we can identify more precisely than a "more probable" one—for the latter has more ways of being realized.

We are thus faced with a very serious kind of absurdity in the second law. If our knowledge of the structure of the world were complete for all times, the probability of one *particular* configuration for each instant would be 1, of all other configurations 0. To say that a system changes from less probable to more probable is merely to say that our assumptions are such that earlier events fall into a class of small frequency (therefore more completely definable) and that later events must be classified with many others from which they cannot be differentiated because our knowledge of them is progressively less able to assign them either to the class of probability 1 (the event which transpires) or of probability 0 (all other possible events). We, for some reason, are generally on the downward slope of Lotka's curve (p. 31). (He plots "number of tickets in urn A after nth draft" as ordinate against n, "the number of double drafts" as abscissa.)

There are two possible reasons for this: (1) The assumptions on which our analy-

sis is based are in some way similar to the conventional type of probability problem —that is, one in which little difference of opinion is possible as to the grouping of possible events. However, we may remember during our own introduction to the calculus of probabilities how easy it was to make an unorthodox interpretation of a problem to obtain a new value of the probability and how by being careless of our hypotheses it was possible to arrive at absurd results—for instance, that there is life on Mars: (10).

(2) The intrinsic nature of *physical processes leads us to prefer this kind of classification* above all others.

Evidently the second law will have a modified significance if we can arrange our apprehension of events in the same way as Lotka does to produce an *upward* trend of the curve (i.e., towards less probable states). Here, however, we are held up until we decide whether or not the physical facts will allow a process analogous to painting half of the cards black, at any arbitrarily chosen instant. We cannot do this if we are interested only in the available energy and arbitrarily name the state of maximum entropy "the most probable." However, if we mean available-in-the-light-of-the-biological-organization-present, or use some other criterion, we *may* transpose the "painting black" to some other point of the process. The peak of the curve (i.e., all black in urn A) will not now appear at the same ordinal number in the series of (double) exchanges and because of the emergence or introduction of organic or psychic wholes in the system may appear later and provide an apparent decrease of "something-corresponding-to-entropy."

By setting aside mechanical work as our criterion, we can perhaps produce a number of peaks (or "least probable" arrangements) dependent on the principle with regard to which the events are classified (as shown later in this paper from another angle). This is G. N. Lewis' idea when he says of the permutations of four playing cards (ace, two, three, and four of spades): "There is no one of the twenty-four arrangements (other than the 1, 4, 3, 2 of whist) which might not be particularly significant *in some other card game*." Or: "There is no such thing as a well-shuffled pack except with reference to familiar sequences." (21).

G. N. Lewis has succeeded in showing very clearly that for a mixture of three different gases the entropy depends on our knowledge (or "information" he calls it) by using either Maxwell Demon considerations or an arrangement of two pistons with differing semipermeable properties in the manner of the Gibbs paradox problem. His analysis proves clearly that "the increase in entropy comes when a known distribution goes over into an unknown distribution," that "Gain of entropy always means loss of information and nothing more," and, very significantly for later portions of this argument, that "It (entropy) is a *subjective concept*." (21).

These considerations have been forcing themselves on us for some time. Schottky (28) makes a distinction (in which Lewis' analysis is implicit) between what he calls "formal" and "technical" entropies. In conversation with the author on the problem of Gibbs' paradox in July, 1929, K. F. Herzfeld delivered himself of the opinion that "reversibility depends on the criterion of separability" and that entropy does depend on our knowledge: "A state of which little is known embraces a large number of possibilities," he said. The entropy of a state then decreases as our knowledge of it increases and, as we concluded at that time, assumptions and conventions must be taken into account as well as Lewis' "information."

I am told by a mathematical physicist that this indefiniteness does not influence the experimental fact. I would answer (1) These considerations influence our interpretation of the facts, render them less able to mislead us and will certainly lead to unsuspected new facts. (2) The experimenter must report his experiment in words and if we wish to know what has happened we had better be clear as to what he means by these words. Kelvin, Clausius, and Boltzmann were not reporting on experiments when they explained to us the meaning of the second law.

It is true indeed that "the universe is running down," but the amount of available energy which will be *successfully* tapped off for human (or living) purposes is not predetermined alone by the intrinsic energy-properties (i.e., thermodynamics) of materials. The available energy in the world, of which Boltzmann talked so poetically, is, from this point of view, as academic as the total amount of (almost entirely) unavailable energy. In a highly organized system, the "available" energy is a function not only of p, v, t, and the state of chemical combination, but of the nature and degree of organization of the matter present. The practical question is: How much of the available energy will we succeed in using? It might all go to waste or we may use varying amounts of it up to the total amount. This question can be answered only by not ignoring the knowledge available, or the organisms and machines to be used. We cannot, of course, give a quantitative answer except in very simple cases, but we can treat isolated systems with these ideas in mind or, at worst, admit this limitation of the physical analysis.

We shall feel more confident about these apparently unsettling conjectures if we go back to the origin of the first law of thermodynamics. Helmholtz says (16):

> Science regards the phenomena of the material world according to two processes of abstraction; in the first place it looks upon them as simple existences, without regard to their action upon our organs of sense or upon each other; in this aspect they are termed *matter*. . . . When we wish to make actual application of our idea of matter, we can only do it by means of a second abstraction and ascribe to it properties which in the first place were excluded from our idea, namely the capability of producing effects, or in other words, of exerting *force* (energy). It is evident that in the application of the ideas of matter and force (energy) to nature the two former should never be separated. . . . Both. . . . are abstractions from the actual performed in precisely similar ways. Matter is only discernible by its forces, and not by itself.

Extending his argument, I would say: Available energy is only "discernible" with the cooperation of mechanisms or organisms "and not by itself." Newton and Galileo successfully worked with the concept of matter in isolation just as physics has up till now successfully separated itself from biology and psychology. Later, to the simple concept of matter in its gross dynamical relationships, Nernst and others have added the further abstraction of "chemical energy." If (quoting Helmholtz again) "we wish to make actual application of our ideas" of matter and energy "we can only do so by means of an abstraction" to which we vaguely refer as organization, life or intelligence, and I would argue for placing these now in the picture that Helmholtz gave us, and that a separation is no longer justified.

MAXWELL'S DEMON AND THE ORGANISM

The possibility that the second law of thermodynamics may not apply to living systems rests mainly on the idea that an organism for some purposes may function in a manner similar to Maxwell's Demon. If we attempt to formulate specifically the function of the latter, for instance, when it divides a mass of gas of homogeneous temperature into two portions of

differing temperature we find the following: (1) the Demon is able to observe certain microscopic features of the system and to base his action on the result of these observations; (2) the outcome of his activity is that the system previously homogeneous with regard to the variable he is observing has now become heterogeneous or differentiated.

We might consider the "purpose" of such a creature to be the setting up of temperature differences. We attach importance to this because in the system thus modified, part of the random heat energy of the molecules has become available for the performance of useful work. From the point of view of the Demon, or regarding this "purpose" alone as a standard to be aimed at, it is reasonable to say that the system in its later states is more highly organized, or, if we have in mind the subsequent application of this organization to performing work outside the system, he might be regarded as having directed some of the uncoordinated motion of the molecules into the coordinated motion, for instance, of the flywheel of an engine. His activities may be summed up by saying that by the selection or choice of favorable events an increase of order or coordination, or a decrease of disorder is made possible.

It is generally believed that this mechanism cannot lead to a reversal of the second law in living matter. Smoluchowsky shows that such an unidirectional opening is not physically possible, either because it is subject to fluctuations itself or because its stability would of itself prevent it from opening. "Eine Gleichrichtung der ungeordneten Molekularbewegung ohne Energieverlust ist auch in beliebig klein dimensionierten Strukturen unmöglich." (Coordination of disordered molecular motion without loss of energy is impossible even in structures of vanishingly small dimensions.) (25) (29). However, it appears that one of the outstanding characteristics of life shows certain similarities to the above. (This was pointed out by Lotka at a meeting of the Cosmos Club, May 31, 1924.) Life modifies the "organization" of matter in two ways: (1) it breaks down undifferentiated food stuffs and reorganizes them into cells and that arrangement of cellular parts most adapted for the purposes of the organism, that is, self-perpetuation and protection, excitability, nutrition, motion, reproduction, etc. (2) The organism acting as a unit modifies the distribution of matter and of large scale objects in its environment with essentially these same objects in view, as for instance when the bird builds a nest or a man his house. The selection or organizing carried out by the organism is not with respect to velocity as in the case of the Demon, but, for instance, with respect to chemical composition, space-distribution, or configurational properties. Unlike the Demon too the selection of the organism may be macroscopic.

I wish to avoid the suggestion that these considerations involve a possible interference with the second law as ordinarily understood, or that the Demon as a quasi-intelligent agency is necessary for the creation of biological organization.

WHAT IS "ORGANIZATION"?

If you ask what specifically I mean by "organization," the answer might be given that the more highly organized system has more possibility of undergoing a given kind or greater variety of experience. (Evidently two terms are necessary for these distinct cases. We might call them respectively "convergent" and "divergent" organization.) It is thus evident that we cannot separate the conception of degree of organization from

what I have called the "purpose" of the organizing agent; that is, the end-state of the system used as reference. We here use the word "purpose" not implying there is some directing agent, vital principle or entelechy which determines the development of the organization. A system will be said to be organized with reference to a "purpose," r, insofar as it is a step on the way toward complete attainment of the final pattern or process, r. It is this postulated final state which we shall call the "purpose." For a germ cell, the "purpose" might be the standard dynamic arrangement of chemical substances and cells so functionally related as to have the properties of the mature organism which the germ cell will later attain.

Consider two states (or complexions), (1) and (2), of a system. Let r be the "final" state hoped for—or used as reference in the calculation—and let φ_{1r}, φ_{2r} be the respective probabilities (in the light of the knowledge available) of the changes $1 \rightarrow r$ and $2 \rightarrow r$, (φ's assumed dependent on the nature of the initial state and final state). If $\varphi_{1r} > \varphi_{2r}$, we may regard state (1) as *more highly organized* than state (2) with reference to purpose (r). The measure of organization here suggested implies, then, the imagined completion of a certain type of process, the end-state attained being a standard against which the degree of organization of other states is measured. A state (1) which has a greater degree of organization than another (2) with reference to state (r) has a molecular arrangement in which the transition to (r) is inherently more probable—that is, attainable either by equally probable transitions from a greater number of equally probable complexions or realizable by a greater number of equally probable transitions from one given complexion. With reference to another postulated end-state (s), however, the probabilities φ_{1s}, φ_{2s} in both cases might be very different.

PROBABILITY OF TRANSITION

This analysis implies a dependence on *probability of transition* about which pure thermodynamics has had little to say. The field of chemical reaction velocities deals empirically with one aspect of this. Raschevsky (26) has suggested on purely physical grounds a general principle in this field:

$$\frac{d\lambda}{dt} = \kappa \frac{\partial S}{\partial \lambda}$$

where λ = the parameter which is changing—pressure, concentration or mass,

S = entropy, and t = time,

and κ is a function for a particular type of change. This equation has not yet been experimentally verified or disproved. Lotka (22, p. 357) infers from biological evidence that an isentropic system tends to change so that the total flux of energy through the system is a maximum. The new quantum theory has also begun to handle such problems.

We might, of course, avoid the difficulty by assuming in simple cases that the probability of a transition is independent of the initial state and dependent only on the probability of the final state reached. This, however, is manifestly unsound. The addition of a catalyst alters the probability of a chemical change though the products may remain the same. Such a suggestion, too, ignores the present idea of degree of organization as a measure of intermediate configurations.

G. N. Lewis (21) has brought forward some considerations in this connection which tend to show that the intrinsic probability of transition from one ele-

mentary quantum state to another depends equally on the initial and final states. Indeed, he believes that, as the relationship must be symmetrical, $\varphi_{ab} = \varphi_{ba}$.

NOTE 1. Probability of transition has no obvious connection with probability of a state as ordinarily handled in statistical mechanics. However, it is possible to imagine in special cases that the a priori probability of a given state is a function of the probabilities or frequencies of the various transitions by which it could arise from other configurations whose a priori probabilities are known.

Let $P_1, P_2 \ldots P_r \ldots$ be the a priori probabilities of various states, (1, 2, . . . r. . .)

$\varphi_{1r}, \varphi_{2r} \ldots \ldots$ the "intrinsic" probabilities of the transitions from each of these states to state r.

Then $P_r = f_1(\varphi_{1r}, P_1) + f_2(\varphi_{2r}, P_2) + f_3(\varphi_{3r}, P_3) + \ldots$ where $f_1, f_2, f_3 \ldots$ are certain functions to be determined.

These P's differ from the p's of Lewis in that the states to which they apply may embrace a number of elementary states or permutations. The present φ's are subject to the same distinction.

The word purpose as applied to conscious animals generally has metaphysical connotations as to the nature of the force impelling the animal toward attainment of its purpose. We do not wish to imply any such finalism or teleology. In ordinary language "purpose" implies some agency which foresees an end and superimposes its will on a system in the light of this end. Such an agent is regarded as interfering with chance. It is not necessary, however, to hold any belief contrary to the spirit of physics in order to regard the final upshot of a synthetic change as if it were arrived at because of known forces whether in physics, biology, or psychology. The concept of purpose used in this essay means simply the final configuration with reference to which other arrangements are judged. The extent of the preference of a given intermediate system (a) for a given end-state (r) is measured statistically by noting the number of times the system (a) develops into (r) in relation to the total number of changes. This interpretation of purpose has been discussed recently by Krikorian. "The purpose of act or object (is) the *expected end-result* of that act or object." (18).

Evidently such an interpretation is just as applicable to a purely physical system as to an organic one. The "purpose" of a dynamical system is the state of least potential energy or minimum free energy. Mach (23) says: "We can conceive of the natural forces of attraction or repulsion as purposes or intentions of nature. . . . It is the purpose of nature, accordingly, to bring the iron nearer the magnet, the stone nearer the centre of the earth. . . . If such a purpose can be realized it is carried out." The "purpose" of the Maxwell Demon and his partition, if such could be realized, is a state of lower entropy.

"Organization" is applied either to structure or to the setting up of the structure. We shall use the word in its static sense and the expression "process of organization" for the latter. We must also differentiate between organization which finds its expression in a certain structure or—as in the physical case—in the fact that this structure is instrumental in realizing a certain process—though in biology these are interlinked.

ENTROPY AS A MEASURE OF ORDER

In measuring organization we must surely bring to light the tacitly assumed criterion of "organizedness." In physical systems we have, in the past, used the free energy or the entropy as a measure. There the reference end-state is the state of minimum free energy or of maximum entropy, but in both these cases the reference end-result is the state of *least* organization and not, as in my suggestion, the

state which is best adapted to, or most highly organized for, the end in view, (which is, presumably, the conversion of all the available energy into mechanical effect). The physical organization also differs from that suggested earlier in this paper in that it can be measured by the free energy available *if* the system changes to the "disorganized" state, i.e. by the use which *can* be made of the organization once set up (if it is so used) not by the *probability* that such use will *actually* be made (which would be the analogue of our "probability-of-change" criterion). For this reason one might adopt as a measure, instead of the transition probability method, the use of a more "generalized" entropy as defined by the Boltzmann formula $S = k \log_e$ (probability). Here, of course, the probability is the a priori probability of the state or complexion, taking all relevant facts into consideration, and the measure is of "disorder" (36). Professor Herzfeld has suggested to me that we use "order" and "disorder" in physical chemistry and "organization" and "disorganization" where bio-psychophysical concepts are introduced and of which a more "generalized" entropy might give a measure.

The *a priori* probability of a state is defined in physics as (Herzfeld, 14)

(1) the limiting value of the fraction of the total time of observation for which a specified state is realized when the time is made longer and longer—which, for a large number of observations, is identical with

(2) the fraction of all the individual random observations for which the system occurs in the desired state or with

(3) the relevant fraction of the total number of possible arrangements of the system, that is,

P_1 = the ratio

$$\frac{\text{number of individual ways in which this state (1) is realized}}{\text{total number of permutations of the system}}$$

It might be better in view of the discrepancies mentioned above between the physical and methods of calculating the probability and end-state to avoid overstressing the analogy with entropy and define the change in the "*r*-type-*dis*organization" as

$$\log_e \frac{\text{a priori probability of (1)}}{\text{a priori probability of end-state (r)}} = \log_e \frac{P_1}{P_r} = \log_e P_1 - \log_e P_r$$

The inclusion of the second term emphasizes the fact that entropy difference is the essential quantity. If we use the Boltzmann constant k for convenience (though it has only formal significance here) this quantity might be written

$$\frac{\Delta \sigma_r}{k}$$

or

$$\frac{1}{k} \text{ ("}r\text{-type entropy" change).}$$

It is assumed here that the reference state is the one of highest organization, i.e., $P_r < P_1$ and $P_r < P_2$. Note however that the definition of "purpose" is such as to admit either less ordered or more ordered as legitimate end-states. This method gives us a scale on which to classify state (1) with reference to the end-state used, as does entropy in physical changes. In physics the entropy change is measured by

$$\Delta S = k \log_e \frac{\text{probability of state (1)}}{\text{probability of state of maximum entropy}} = k \log_e \frac{P_1}{P_{\text{max.}}} = k \left(\log_e P_1 - \log_e P_{\text{max}}\right)$$

In thermodynamics the denominator of the fraction is often omitted. The more highly ordered state has the smaller entropy and therefore increasing order means decreasing S. Similarly the greater

is σ_r the greater is the "*dis*organization" with reference to (r).

Can this treatment be reconciled with our previous definition of "more highly organized?" The earlier method makes the state of greater "r-organization" that for which the transition to r is more probable. If (1) is "more highly organized" than (2) or $\varphi_{1r} > \varphi_{2r}$ (transition probabilities) we should expect the "r-type entropy change" from $1 \rightarrow r$ to be less than the "r-type entropy change" from $2 \rightarrow r$. That is,

$$k \log_e \frac{P_1}{P_r} < k \log_e \frac{P_2}{P_r} \text{ (a priori probabilities of states)}$$

or $\log_e P_1 < \log_e P_2$. That is $P_1 < P_2$ if the two definitions agree.

In a general way we can see that the more probable is the change $1 \rightarrow r$ the nearer must the configuration (1) be to (r) and thus "further" from the chaotic or most probable state. On the other hand we must not ignore that there will be usually less complexions of (1) to change *from* than of (2) and thus the a priori and transition probabilities will operate in opposite directions. This deduction agrees with Lewis' equation for elementary states: $P_a \varphi_{ab} = P_b \varphi_{ba}$. It should be noted that these methods are most relevant to systems which are still a long way from equilibrium and in which a degradation of free energy is going on.

It must be admitted that the above is only tentative. The choice between the two methods, if they have any value, must await the further development of physical chemistry and statistical mechanics. Until then we cannot apply them in other than the most general way to biological problems. At present I incline to the belief that the transition probabilities offer the more fruitful approach, but the entropy method has the advantage of tying up with a branch of theoretical physics which is already thoroughly developed. The considerations of Note 1, p. 151, suggest the possibility of harmonizing the two methods.

In another place (37) these methods have been compared on a sample type of linear organization (four playing cards which continuously change from one permutation to another by simple exchanges). I find that the transition method is definitely better as it reveals the specific nature of the organization. If we look for examples in biology, they are so complex that any reasonable numerical estimate would be impossible without a great deal of investigation. In all cases the "entropy" gives a static measure of the organization. The second method gives a "historical" estimate as dependent on the factors governing changes, both past and future, in the system.

ILLUSTRATIONS OF THE TWO MEASURES OF ORGANIZATION

The nature of the difference may be understood by reference to the following cases:

(a) At a certain point of time, two zygotes, of species amphioxus (α) and species of star-fish (β) are apparently identical in chromosome structure and have undergone cleavage the same number of times; yet if we are interested in the mature starfish as end-state the blastula (β) is "highly organized" towards that end and blastula (α) not at all. An observer, ignorant of the chemical differences, would conclude that the two systems had equal "entropy" yet the future event shows that the organization is fundamentally different. In another case, the chemical constitution of the chromosome substance might be identical and the genetic difference dependent on differences in macroscopic structure. If an example of the latter could be found it would be

a more convincing illustration of the value of these methods.

(*b*) Consider the following physico-chemical systems:

(1) suspensoid sol,

(2) electrolyte solution, which can coagulate it,

(3) emulsoid sol, protective to (1).

If the solutions are mixed in the given order, there is precipitation; if in the order 1, 3, 2, there is no coagulation. The initial entropies of the components are identical in the two cases. Entropy does not indicate the difference in final state produced by difference in time-order, just as it does not indicate why the result of mixing d-glucose and yeast should be alcohol and CO_2, though l-glucose, under the same conditions, does not change.

(*c*) Consider the "entropy" of a particular living animal at a certain location. This will depend among other things on probabilities P_a, P_b, P_c . . . etc. that certain food substances (*a*) (*b*) (*c*). . . are present there and on the density of population of the species in that region. We should, however, come to a wrong conclusion as to the survival of the living system if we ignore the ability of the animal to search elsewhere for a missing food-stuff or to get along with a substitute.

(*d*) The organic property of interfering in subordinate mechanisms, when the integrity of the whole is endangered, or the interference in local processes by nervous impulses or hormone activity (initiated by central activity), which alter the conditions of selectivity, offer other examples for testing these methods.

(*e*) On another level we have the interference of psychological or cultural facts or stimuli in determining material distribution of the higher vertebrates (as we shall show later) and of their cultural apparatus.

THE EXTENSION OF THE ENTROPY CONCEPT

It is evident that a revisal of our prevailing method of analysis of systems will be necessary if the new measure of organization is to be introduced. This method will, however, have the great advantage of bringing to light the *significant* aspect of a structure, not simply its usefulness in producing mechanical energy. Even in the simplest of physical machines (40) the efficiency or mechanical advantage reveals only one aspect of their usefulness. The simple lever can multiply an applied force but often its real function is to change the point of application or direction of a force or moment. A lever which merely changes the point of application of a force does nothing from the point of view of energy or of entropy, i.e. from the standpoint of the first and second laws. Yet it may effect a change in biological organization. If we are considering the muscular system of an animal the entropy is eminently satisfactory but for the digestive, circulatory or reproductive systems, the dynamics is only instrumental and gives a misleading indication of their organization.

All that is necessary for our present argument is that we agree

(1) that if we take into account the biological and psychological organization of a system the probability will assume a new value and that a new kind of "entropy" will be possible, whose actual value is less important in the first place than the modification which it will introduce into our thinking. Thus it might be maintained that the "entropy" of the system on which the Maxwell Demon operates changes on the addition of the Demon to the system. His presence increases the probability that the system will become differentiated, even

before a single molecule has crossed the partition. This modification of classical entropy does not in itself involve acceptance of the "disorganization coefficient" above. Here the reference state remains as in physics but the influence of supramolecular organization in modifying the frequency distribution of states is taken into account. To quote Lotka (22, p. 121): "a dynamics of systems comprising living matter must necessarily take account. . . . of this faculty" (the power of cheating chance).

(2) In the second place if we can use a different reference end-state another change in the "entropy" may result. The entropy concept thus extended includes entropy of "organization-for-energy-availability" as a special case.

That the value of the entropy does change in consonance with, or is a function of, the assumptions on which the probability is calculated is evident, as we have indicated above, from the new quantum statistics of the ideal gas formulated by Bose-Einstein or Fermi-Dirac (1). In the former the criterion as to what constitute separable complexions is changed—the distribution being defined in terms of *numbers* of particles in the compartments of the phase-space rather than of the more specific permutations. In the latter the Pauli exclusion principle limits the number of allowable cases and thus further departs from the classical probability. It is true that physicists can say definitely the kinds of systems where the Bose-Einstein (for radiation) or the Fermi-Dirac (for electrons and protons) statistics apply and those for which the classical statistics are adequate (at higher temperatures). However in borderline cases the validity of one system or the other is found to depend on *conventions* as to what constitutes an allowable measure of disagreement between theory and experiment.

Similarly a statistical calculation which allowed, for instance, the action of intelligent interference would necessarily arrive at different values for the entropy. This has already been examined by Szilard (32). The "measurements" of which Szilard speaks and which he regards as a prerequisite for possible entropy-decrease are similar to the "observations" of the Demon referred to earlier. I differ from Szilard in believing that the observation need not be quantitative or even crudely metrical in order to facilitate changes in the degree of organization. Szilard concludes that, as in other cases, there is compensation in the making of the observations.

The indefiniteness of entropy thus admitted into statistical mechanics is reflected in thermo-dynamics. The integrating factor $\frac{1}{RT}$ used for the equation, for a perfect gas,

$$dQ = C_v dT + \frac{RT}{V} dV$$

leads to the $\int \frac{dQ}{RT}$ which we name "entropy." However the number of possible integrating factors is infinite and hence there should be an infinite number of (physical) expressions allied to entropy. Entropy is the simplest of these (2). Further the integral $\frac{dQ}{RT}$ includes an integration constant which can be varied according to the reference state (from which entropy differences are measured) and for this reason also the entropy can therefore assume several values. This gives some justification for my retention of the term "entropy."

The expressions obtained by using other integration constants than $\frac{1}{RT}$ do not, it is true, differ essentially—as regards their influence on the way that laws are formu-

lated—from entropy as ordinarily understood. The operations by which it is obtained are essentially the same and show the same indifference to factors which would claim the attention of a biologist or of a psychologist—factors which emerge from a system at higher levels of organization in virtue of that organization—factors which could not be predicted or identified from an examination of the elements viewed in isolation or without knowledge of the relationships into which they enter in the configuration. A good analogy in physics here would be the use of "regeneration" in a radio circuit. The current in the grid circuit depends on the geometrical form of the rest of the set-up.

KEY ACTION

An objection might be raised to our analogy between the selecting functions of the Demon and of a living organism—that the Demon acts *intelligently* so as to interfere with unfettered statistical effects. However, the duties of the Demon are both limited and mechanical as were those of Humphrey Potter attending to Newcomen's engine. It may be (we cannot escape the suggestion) that there are processes controlled by selecting agents, purely mechanical or physico-chemical in nature, which are not open to the objection that intelligence is required. Such agents would appear to produce an "unshuffling" with reference to a predicated "purpose." The valves of a steam engine or an internal combustion engine might, I think, be examined from this point of view. The valves perform a function similar, but not identical, to Maxwell's Demon in controlling the conversion of the uncoordinated energy of the expansion or explosion into coordinated motion. They select just the right instant for allowing the steam to impinge on the piston head, just as the Demon selects the right instant for withdrawing the partition. The intervention is timed but the selection is made to depend on the value of a *macroscopic* parameter. The relation of an engineer with his hand on a switch to the machine controlled by the switch, or of a nerve cell to the muscle it excites, will be seen on reflection to present similar features. We have, in the past, ignored this function of automatic or semi-automatic regulators which I shall call "key" or "trigger" action. A key action is one which initiates or directs the liberation of the free energy in ways that increase the "organization" of a system or redirects already existing processes to render more probable an organization in harmony with a formulated "purpose." In the conventional discussion of Carnot's cycle no attention is paid to the means by which the perfectly conducting and perfectly insulating walls are to be interchanged at just the right instant. This omission in the physical method renders it unable to deal with such mechanisms in organisms where coordinated intervention is universal, and illustrates the habit of physics of making certain ideal abstractions from the complex of events.

It might be pointed out that the valve, being a product of intelligence, is open to the same objection as the Demon and that the nerve is part of a living organism of whose rôle in this action we cannot be sure. We must, therefore, look for selecting processes in inorganic systems. Where in non-living matter do we find "organization" increasing, and is there in these anything similar to *key* activities?

An examination of inorganic selecting processes shows three distinct types:

(1) In filtration, diffusion through a semipermeable membrane, magnetic separation, fractional distillation, (in short the numerous methods of separation used

by the chemist), we have a separation which results in an apparent decrease of entropy. In all these cases, however, this decrease is compensated by a greater entropy increase in some other part of the system—by the decrease in the gravitational, concentration, or magnetic potential or the degradation of free heat energy. The separating agency in each case is resident in the intrinsic physical properties of the system: porosity of the filter or membrane, differential volatility of the vapors, differences in magnetic permeability, etc. (separating processes).

(2) Physical chemistry gives us many examples of periodic structures which appear spontaneously and which simulate biological organization. In the formation of certain crystals, droplets and surface films, in the chemical "garden," (produced by throwing a crystal of one soluble salt into a solution of another with which it forms a colloidal precipitate) in colloidal gels, Liesegang's rings, the fern-like crystals of lead produced in the rapid electrolysis of a concentrated solution of a lead salt or by slow reduction, the beautiful patterns in many wave-motions and in hydro-dynamic systems generally, as shown for instance in photographs of bullet-tracks. These are spoken of as static patterns but even more striking are four-dimensional configurations in flux as, for instance, wave-motion, vortex rings, or the pleasing arrangements of partials from a pipe or violin which constitute a musical note. Of the same nature, too, is the mechanism by which complex organic molecules build themselves up under favorable conditions from simple substances—as when formaldehyde gives rise to a hexose, or later to glycogen, starch and fats. All of these again are produced with entropy increase, in the ordinary way. They are the obvious examples of a much more general phenomenon by which the unimpeded action of electric, magnetic, molecular and gravitational fields together with the discrete structure of matter produce significant organization. The cases in which we are interested are, of course, those in which the organization is of value not in a spectacular way but as instrumental in facilitating an objective (biological) result. There is no reason why the structural units and component "mechanisms" of an organism should not arise and be maintained in response to other configurations of the same forces—(structurating agencies). The gap between these cases and those in which the same forces impel the organism in search of its food or develop its cells into useful patterns is, I believe, one of degree of complexity.

(3) Those selecting agents of which the valve is typical, influence the nature and speed of a spontaneous process. Here, again, the driving agent is the free energy of some part of the system and the entropy increases. This class covers the whole field of chemical catalysis, homogeneous and heterogeneous, hysteresis, enzyme action (microscopic) and (macroscopic) valve, choosing or gauging mechanisms of human invention. These control the rate of increase of entropy or determine in what direction the system will expend its free energy—among other things, whether the free energy will express itself first as mechanical energy or merely dissipate itself as heat. A significant feature of these is that small quantities of energy control the fate of much larger amounts. Evidently such agents, which alone of the three classes merit the name of "key" or "trigger" mechanisms, influence the probability of transition. A very instructive illustration of this principle is to be found in chemical reactions. The rate of a gaseous bi-molecular change is controlled by the fact that only molecules

whose energy is above a definite amount are "selected" for reacting. Such key-organization may either be active or only latent as in spores and other dormant biological systems.

THE NATURE OF THE ORGANIZING AGENCY

These phenomena are of importance in the light of the considerations of this paper in that they indicate some of the means by which organization of biological importance can be produced in development, growth or anabolism and emphasize the necessity of incorporating a measure of their organization into our physics and chemistry if these are ever to be able to tell us more as to the essential nature of organism than the entropy concept does at present. In organisms themselves, Lashley has given a clear insight into the nature of organization for one class of structures. "Cerebral organization can be described only in terms of relative masses and spacial arrangements of gross parts, of equilibrium among the parts, of direction and steepness of gradients and of the sensitization of final common paths to patterns of excitation. And the organization must be conceived as a sort of *relational framework* into which all sorts of specific reactions may fit spontaneously. . . ." (19). In the light of this, too, it is quite unnecessary to postulate either a "vital force," "master reaction," some very rare fluctuation or the passage of great periods of time to make possible the so-called "infinitely improbable" structures of living matter. The organizing agency is not centralized as philosophers, such as Rignano and Driesch, imagined, but is diffused throughout the whole organism in the form of residual valence, electrostatic and electromagnetic field patterns (static and dynamic), chemical affinities, potential differences, surface tension, adsorption, and in the *speeds* with which atoms, molecules, ions, colloid particles, cells, tissues and complete organisms respond to their influences.

The nature of the mechanisms which maintain an approximately constant internal environment for the cells of a metazoon for instance, is now traceable in considerable detail to known physico-chemical processes (Cannon, 4). All three types of organizing process are probably involved in these functions. Physiologists like J. S. Haldane admit the value of these considerations but are unable to find in them a clue to the regulative action of the organism as a whole on the operation of individual mechanisms or equilibria. Only two years ago, F. G. Donnan expressed the view of the physico-chemical mechanists—who are not to be accused of any hankering after vitalistic mysticism—when he said: "The harmonious and dynamic correlation of the various organs and tissues of a living organism ever confronts us as one of the great mysteries of life." (8). A. V. Hill talks of the living cell as "a complex organized system of enzymes, interfaces, potential and osmotic differences, chemical substances *infinitely improbable in the thermodynamic sense* and yet existing in the steady state so long as free energy is available to maintain the organization." (15). The use of the word "improbable" in this connection makes the assumption that all events are equally probable and independent of each other, but this is evidently not the case.

O. Meyerhof expresses the same difficulty in these words: "One cannot understand how life can maintain itself for such long periods of time when it must, from a physical standpoint, always remain infinitely improbable." (25). Henderson (13) pessimistically concludes likewise that "the riddle surpasses us and . . . the contrast of mechanism with

teleology is the very foundation of the order of nature." I am inclined to believe that a combination of certain contemporary viewpoints makes us more able to comprehend these improbabilities and the possibility of a "chemical structure so designed as to perpetuate a perishing existence." (Leathes, 20). I shall outline below the elements on which this synthesis might rest.

Another aspect of the same problem is expressed by Meyerhof thus (25): "There is no thermodynamics peculiar to morphological processes. . . . For the formation of cell-structure, no special work is required. There is therefore no '*Strukturenergie*' into which chemical energy is converted during growth. Further, differentiation possesses no energy-equivalent nor is it correlated with an entropy decrease of the system." This statement illustrates well, I think, the tendency to over-estimate the value of energetic considerations in the analysis of a configuration. The free energy of the glycogen oxidation, glycogen→lactic acid breakdown which A. V. Hill regards as the *sine qua non* of the maintenance of structure and similar changes are the only aspects of it about which pure thermodynamics can inform us. Physical entropy does not provide a very delicate measure for structure if that structure is not adapted to or designed for making immediately available the energy of the materials of which the structure consists. For only an infinitesimal fraction of the free energy used in development or anabolism expresses itself morphologically.

THE PHYSICO-CHEMICAL BASES OF ORGANIZATION

I suggest that the fundamental difficulty is none of the above, but this: that we cannot understand yet by any symbolism in common use the details of the method by which a dynamic structure can, so to speak, react back on itself so as to maintain itself intact or induce in neighboring lifeless matter an organization similar to its own or divide itself in such a way that the parts have latent in them the same functional possibilities as it has.

NOTE ADDED MAY, 1931: This, however, has already been done by Dr. N. V. Raschevsky in a rigorous mathematical analysis for the special case of a liquid droplet which grows by chemical interaction of substances dissolved in a surrounding medium. The work has been published in a series of articles in the *Zeitschrift für Physik* and is to appear in summary form in an early edition of the new periodical "Physics." See also the "Science News Letter" for May 9, 1931.

We have, however, some clues in, for instance, chemical auto-catalysis, in which a reaction feeds on itself because the products stimulate increased reaction of the reactants, or in Leonardo's candle flame, which shows all three of these phenomena. Life is a process or method, an integrally connected series of operations, an infectious principle of using stored free energy. The flame at any instant passes on to the flame at the next instant the secret of combustion (no secret now, when we understand to some extent such simple processes).

The picture becomes much clearer and the strain on our imagination less if we try to keep in mind simultaneously the following considerations: To begin with, the three aspects of physico-chemical structuration outlined above.

(1) Certain arrangements produce chemical separation or differentiation. Such processes may occur on a geological scale without the assistance of human supervision. Thus, the colloids of the Mississippi are precipitated when the river meets the high ionic concentration of the sea.

(2) Physico-chemical space-time pat-

terns may appear spontaneously (physicochemical structures).

(3) The intervention of catalysts or valves (key agents) may control the rate of changes, or, more generally:

(4) We underestimate perhaps the constructive possibilities of a consistently operating selecting mechanism that "introduces order into the movements and spatial relationships of foreign molecules (and bodies) in its vicinity;" (e.g., in membrane equilibria and the CO_2 gas-liquid equilibrium in the blood). In this class, too, we must include emergent selecting mechanisms on higher levels such as the kidney. A very significant group of such mechanisms are those whose free energy changes by only small amounts but which may affect the mode of decrease of free energy in large amounts of other materials (the ductless glands). One might here suggest the rather extravagant concept of "available form" to emphasize the morphological aspect ignored when we talk of available energy alone.

(5) The specific nature of the chemical substances constituting some mechanisms will also cause a chemical selectivity. Thus it is possible that there is some correlation between the specific proteins in the chromosomes of a germ cell and the large-scale structure which arises from it through development (34).

An analogous fact which suggests some such mechanism is seen in the "biochemical adaptation" by which the proteins of antisera produced in horse blood are found on injection into dogs to become rapidly indistinguishable—by precipitation tests—from normal canine proteins (24). By other tests, this protein can be shown after three months to have retained some of the specificity of its origin. The "biochemical hybridization" of bacteria by growing them for several generations in dilute solutions of known protein—shown by their agglutinability by precipitins specific to this known environmental protein—is another arrow pointing in the same direction (24). Apparently immunology has progressed further than other branches of biology in uncovering the specific chemical substrata of larger scale phenomena. It seems possible that this functional or structural specificity of a molecule to a particular species, is not confined to immunology but may also be found in embryology and genetics.

The "plans of the invisible architect," to use a phrase of Donnan's (8, p. 1561), are written in chemical molecular-structure—a language with a great wealth of descriptive power, as the latest edition of Beilstein shows. What I have in mind is that the functional possibilities of a germ cell will be implicit in it not as if it were a "model," on a smaller scale, of the mature organism, but in the same way as the nature of chess games is delimited by the rules which we make to govern the motion of the pieces; or, from another standpoint, as the distribution of population and industrial organization of a country is to some extent implicit in the location and extent of its natural resources, its climate distribution, its means of communication, topographical relationship to other countries and centers of industry and, in another sense, in the racial characteristics of its people, the current world picture, and the state of science, invention and religion. To use Donnan's own figure, the morphological aspect of a completed building is potentially present in the schedule of objective engineering requirements, location, costs, preferences of his client and of himself, which direct the work of the architect when he begins on his plan. These form the analogue of the germ cell organization. The embryo at a later stage might be said to be analogous to the completed plan itself.

(6) On a still higher level of organization, we have those tissues in the embryo which, when transplanted, do not adapt themselves to their new surroundings but retain their own character and force the neighboring tissue to follow them. The work of the "organizers in development" (30) has a functional similarity to selecting mechanisms and to the candle flame.

(7) The variety and large number of such qualitatively different cooperating devices found in many organisms might lead us to expect the emergence of processes of greater range and integration than in simpler configurations as, for instance, in the transition from atom to molecule.

(8) Finally, the *Gestalt* principle shows the universal tendency, even in physical systems, as Köhler continuously insists, towards organized wholes which cannot be subdivided without altering the organization throughout its whole structure and which show *unique* properties. "When two atoms come into their sphere of mutual influence. . . they either separate again or they form an orderly molecule—an *architectonic structure*—without the aid of any arrangements *ad hoc*." (17). The static charge on a conductor distributes itself in such a way that the intensity at each point is functionally dependent on the charge at all the points on the surface. The current flowing through any one conductor of a network is similarly dependent on the distribution of current in all other conductors of the network. A complex molecule like camphor, nicotine, or chlorophyll has also this quality of inviolable unity.

We pour oil into a liquid with which it does not mix. In spite of the violent interaction of the molecules at their common surface, this surface remains sharply determined, not by any arrangements enforcing this orderly distribution, but just by the play of surface dynamics between the oil and the other liquid. If the specific density is the same for both liquids, these surface forces will change the distribution until the oil forms a regular sphere swimming in the other liquid. . . . *Dynamical interaction, undisturbed by accidental impacts from without, leads to orderly distribution, though there are no special regulative arrangements.* (Köhler, 17).

In illustrations such as these, we can see the coherence and unity of wholes which depend on the simplest of forces. How much more effective and comprehensive organization may we expect if the more complex agencies described above combine in one configuration, aided perhaps by the directive tendencies of the previous successes of the configurations to which they are genetically related and by the high individuality with which its protein, carbohydrate, and lipoid molecules have been stamped by the history of the individual and of the species? The term "biochemical echoes" coined by Manwaring to describe this "imprinting" process in the formation of antibody colloids might well be extended to all cases where the experiences of the individual have been retained in his tissues. "Biological memory" (to use Rignano's expression) is both configurational and physico-chemical—the former blanket term covering our incapacity (as yet) to analyze, systematically, structure above the level of the larger organic molecules.

If and where there is an *inheritance* of such echoes, here is a likely place to look for evidence—in the modification of nuclear substances (possibly proteins, nucleic acids, lipoids, or polysaccharides) by the blood which nourishes the maturing germ cell. I doubt if the theoretical objections of Weismann and his successors could be found to be valid for some such mechanism. At any rate, it is certain he did not have this kind of "architectural shorthand" or intramolecular preformation, in mind when he wrote. Genetics assumes a dif-

ferent aspect when we take into account these intrinsic (biochemical) properties together with Woodger's "relational" (i.e., essentially configurational) properties, especially as the latter will change continuously during the whole course of development and (possibly) react with and modify the former as they change. We must be prepared to reexamine some of the central heresies of biological and medical thought in the light of this idea (34).

These same considerations may remove some of the obscurity about the origin of life which Meyerhof talks of as a "point of accumulation of improbable states." (25). The series of fluctuations necessary to produce life now appear less improbable, for the favorable fluctuations may be assisted and captured at each step by one or more of the selecting agents. And the "fluctuations" need not be microscopic only. Donnan's difficulty in connection with this theory—"What is there to stabilize and fix this rare event when it occurs?" (8)—is non-existent, for the essence of the rare configuration in which we are interested is its property of self-perpetuation.

The suggestion that I wish to make then is that all material organization, whether living or lifeless, has its roots in the same facts and that the symmetry and beauty of the products of the synthetic chemist, of geological formations, of trees, of flowers and of young girls are in essence traceable to the same kinds of designing agents.

Since first outlining the above ideas, I have found the same thought expressed by Tyndall in 1867 (35). He has just been discussing the formation, by electrolysis, of lead and silver solutions, of beautiful and striking fern-like structures. His comments must have occurred, though in less succinct form, to anyone who has performed the experiment (see, for instance, the illustrations in reference 33).

> These experiments show that the common matter of our earth, . . . when its atoms and molecules are permitted to bring their forces into free play, arranges itself, under the operation of these forces, into forms which rival in beauty those of the vegetable world. And what is the vegetable world itself, but the result of the complex play of these molecular forces? Here as elsewhere throughout nature, if matter moves it is force that moves it, and if a certain structure, vegetable or mineral, is produced, it is through the operation of the forces exerted between the atoms and molecules.
>
> Latent in these formless solutions, latent in every drop of water lies this marvelous structural power, which only requires the withdrawal of opposing forces to bring it into action.
>
> The solid matter of which our lead and silver trees were formed was, in the first instance, disguised in a transparent liquid; the solid matter of which our woods and forests are composed is also, for the most part, disguised in a transparent gas, which is mixed in small quantities in the air of our atmosphere.
>
> . . . . And just as the molecular attractions of the silver and the lead found expression in those beautiful branching forms seen in our experiments, so do the molecular attractions of the liberated carbon and hydrogen find expression in the architecture of grasses, plants, and trees.

THOUGHT AND ORGANIZATION

I have purposely avoided the objection that the *intelligence* of the Demon is significant in his activities, but in the light of the above considerations it will surely be agreed that if we endow him with a less mechanical reaction pattern (or "habits") his organizing potentialities will be still further enhanced. This leads us to the conception that intelligent thoughts and ideas are themselves, or are parts of, selecting, key or trigger mechanisms, whether they act on single molecules or on aggregates of molecules. Effective thought represents increase of organization of idea-symbols with reference to a given problem, and if we consider the thinker and his ideas *in conjunction with the system on which he proposes to operate*, then, on a mechanistic-behavioristic basis one might say that a successful idea, indeed any act of creative thought, intro-

duces not only potentially but actually an increase of organization—probably, indeed, of the total molecular system of the thinker-ideas-materials. The change appears to be largely confined to the central nervous system and muscles of the thinker. We generally do not think of the change in connection with the rest of the system, but that is because of the atomistic viewpoint that we are trying to unlearn. Can thought then produce "entropy" decrease not only in the brain substances as Eddington supposes (he allows, 9, p. 313, the mind the ability to "tamper with the odds on atomic behavior in the brain") but also in the so-called material environment?

Organization of physico-chemical "Gestalten" consists in one sense of a linking up of fields of force between the atoms. Organization produced by mental activity will also be found to be expressible in terms of connections or bonds. Dewey (7) regards thinking as finding the missing link to place between separated or discordant elements, in particular the link that will lead most probably to the desired result, or somehow diminish the separation or discordance. All we are doing in our analysis is to give to these intangible linkages, or better—relations within the configuration, an importance comparable to that attached to, say, lines of force or potential energy in the naive physical view of the world. Many physical concepts, for instance, lines of magnetic induction or orbits in the Bohr atom, are abstractions of no more and no less reality. The justification for the statistical measures of organization which arise out of the present paper and which are applicable in principle to psychology and the social sciences lies in the fact that this arrangement of thought linkages, whether regarded as "mental" or molecular, can produce predictable and observable results no less tangible than induced currents or spectral lines. The change in the emphasis suggested here is in harmony with the spirit of Gestalt psychology.

This discussion approaches the concept of the Gestalt from the opposite direction to that familiar to the psychologist. The configurationists do not yield to the physicist that the phenomena of pure physics have any superior claim to reality. To quote from Helson: ". . . . the fact must be mentioned that thought structures are real in every sense of the word. Thinking logically is not the result of blind chance, of frequent associative connections or a matter of caprice. For the results of thinking depend not only upon thought, says Wertheimer, but *also upon the objects thought about*." (12, p. 56). Our analysis amplifies this opinion from a new angle.

The occurrence of the thought of electromagnetic induction in the mind of Faraday affects an important transformation of one space-time pattern (1) into another (*r*).

(1) Certain isolated and undifferentiated ore deposits of copper, iron, graphite, etc., plus an unenlightened investigator.

(*r*) their later arrangement in the form of generators and electro-motors and use for the production of electric power, plus a gratified or defunct Faraday.

The thought or insight is something he can reproduce, which is probably imprinted and perpetuates itself by physico-chemical changes on certain cells (presumably) in his muscles, brain and central nervous system and later perhaps in the molecules of the written or printed word. It therefore increases the material organization of (1) with respect to (*r*) considered as a "purpose." Taking the physico-chemical system (Faraday + these parts of his environment containing the copper, iron, etc.) as one unit and isolated

for convenience, in the manner familiar in thermodynamic reasoning, from contiguous bodies, it is evident that the organization of this unit becomes greater in a minute but very vital respect the instant the intuition appears in the mind of Faraday. In short, can we not say that the "generalized entropy" (as we have described it) decreases during thought in which decisions are made? There is nothing in this analysis to show that the classical entropy decreases. From another point of view, the arrangement simply becomes more "known." However, the second law takes on an indefiniteness it did not previously have.

The instruments of the organization process may thus be resident in the special distribution or physical interconnections of matter, as for instance in telephones or protoplasm; but also and most significantly in the idea or theoretical scheme which can unite in a new way distinct, inharmonious or heterogeneous material—whether inorganic or living.

The nature of the influence of thought on organization becomes more easily apprehensible if we abandon the mechanical-atomistic standpoint and, using the Gestalt concept, frankly admit the morphological aspects of experience into theory. "The inner necessity pervading the entire structure does not come from the mere relation of part to part. . . . but rather springs from the peculiar nature of the whole which is more or less autonomous." (12). We are helped in this by our very uncertainty as to the location of the Gestalt. "There are physical, physiological, biological and psychological (and logical) structures possessing properties *not compoundable from their parts* and hence to be regarded as configurations (p. 364). . . . so it is impossible to fix precise meanings for the concept of *Gestalt* when we examine it closely in its various uses in the configurational literature." (p. 368) (Helson, 12). By treating the psychological fact as an integral part of the physical picture, we help break down the barrier between these apparently conflicting interpretations.

There is a weakness in the above analysis with its apparently sudden change in the organization. The "insight" presupposes a discontinuity ("*Einschnappen*") which does not, in fact, occur. The insurgent factor is regarded as a foreign element which suddenly and mechanically intrudes itself upon a structure more or less unprepared for it. As matter of "fact," however, the apparently "improbable" emergence of an intuition, as indeed of all configurational integrations, is due to the growth or decay of the pattern according to definite laws which would be clearer to us if we could discover all the factors operating and adequately apprehend their interconnections. Here again as with the "accidental" first beginnings of life, the outcome is inevitable and explicable. The thought "emerges" from the Gestalt as a necessary result of the presence in it of the elements concerned in their given relations much as a new complex organic compound appears in the mixture of materials from which it is synthesized by the chemist. For the Gestalt is a *dynamic* organization in constant flux. To quote Helson again: "Configuration connections are not the results of frequent associations or blind juxtapositions, but are inner bonds imposed by the demands of the total structure." (12, p. 54). And at another place: "Configurations become more articulate or more finely structured, simpler in form, more precise or less definite, poorer in form or chaotic. What is to be singled out. . . . will depend upon. . . . a number of. . . . factors like coherence, persistence and impressiveness. In gen-

eral, the progress of configurational change is from chaotic groups of elements, at the one extreme (really a minimum of 'structuration') to more and more complex structures." (12, p. 369).

From one point of view, the "entropy" of all the coal and oxygen in the world might be regarded as changing at the time of James Watt's invention by a small but finite quantity. The "entropy" of any given mass of coal changed by an amount which depends on the probability that that mass will be brought into connection with an engine. The existence of the engine (or of the idea of the engine) apparently increases the chance of our using the coal for producing mechanical work; that is, according to our limited insight into history. Looked at after the event, the engine, though "infinitely improbable," was the inevitable outgrowth of the contemporary world and its instrumentality in unlocking free energy was certain.

It might be well to modify the above treatment in the light of Whitehead's picture of the organism (38). We have assumed a succession of instantaneous three-dimensional configurations with no overlapping, but organism cannot be separated from the concept of functioning: the successive events constituting the organism themselves form a configuration in space-time. The same idea is applicable to our heterogeneous system—organism plus material environment. We should really reread the above as dealing essentially with a four-dimensional configuration with less emphasis on the serial relation of "simultaneous" snapshots.

THE USES OF INTUITION

I do not wish it to be supposed that I exaggerate the value of this extension of the thermodynamic method. With Woodger (39) I believe that the development of special ways of thinking about the facts in terms of suitable symbolism promises more in the end. However, that is no reason why the physicist should not try to bridge the gap and to present the biologist with his physical tools in as effective a form as possible.

There is a school of thinkers in psychology, sociology and history (Klages, Hyde, Friedel) which suggests that we may be premature in expecting the same kinds of verifiability and reproducibility there, that we are accustomed to in physics. An intelligent use of intuition by the most informed and profound minds appears to have more significant contributions to make to complex problems than the methods of symbolic analysis or controlled experiment alone. Thus Goethe and Nietzsche were good psychologists. In the deeper regions of truth, universal consent is not a very helpful guide. Indeed have the fundamental theoretical developments of science ever rested on widespread understanding? In virtue of the similarity of some of its problems to those of psychology, it may be that this concession is required in biology. There is nothing essentially unscientific about such a procedure, for the judgments of such men can be formed in the light of carefully analyzed experience. Also our agreement as to the truth of mathematical propositions, on which so many physical proofs are based, is largely intuitive in origin. I am arguing for the admission of intuition at another point in the chain of logical connections, where it may have equally important results.

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DEATH AND ITS CAUSES

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PART I. DEFINITION OF THE CONCEPTION OF DEATH

THE problem of death and of the processes which bring it about has attracted the attention of naturalists during the last twenty years. This attention was due, on the one hand, to the appearance of some new papers devoted to the reproduction of Protozoa, and, on the other hand, to the publishing of several experiments on the artificial culture of tissues and organs. In the monographs dealing with the problem of death, it was united with the problem of senescence. Their authors therefore considered exclusively the problem of so-called natural death, because it was of special interest to the widest circles of readers. In order to understand death it is, however, necessary to consider a more general problem dealing with the process of death independently of whether it is only a consequence of growing old, or whether it is produced by some direct harmful effect of the surrounding medium.

Death never occurs instantaneously. The problem of death involves, therefore, the problem of its gradual development or necrobiosis, which may result as well from normal life as from latent life or anabiosis, if it lasts for a long time. In this part of my paper I shall try to define the conception of death in its general meaning.

The common-sense conception of death

In the common speech death is considered as an irreversible loss by the organism of the property of being alive, that is of showing growth, development, metabolism, movement, and reproduction, which, in the total, form life.

Using this simple definition of death we experience difficulties however, if we consider the death of multicellular organisms. Indeed, life is not anything restricted to one part of the organism; it represents a summarized action of innumerable single processes proceeding in different cells of the organism. As single cells die at different times, one is not certain what moment of the necrobiosis should be defined as death.

In order to avoid this difficulty, Jores (1910) proposed to define death as a complete stoppage of life activities in every part of the body. On the other hand, it is well known that single cells can die in a quite healthy organism. A considerable number of scientists who investigated the process of death (Cohnheim, 1882; Ribbert, 1908; Verworn, 1922; Minot, 1908; etc.) arrived therefore at the conclusion that death represents only a phase of the development of an organism. According to Lipschütz (1915), death is the last link in a long chain of changes which are carried out in a living organism. Schäfer (1913) affirms even that death is the last act of life, while Tangle (1916) considered it as a necessary part of every life, because, according to him, every physiological process becomes possible only when some amount of "living substance" is annihilated. We see therefore that no agreement was reached in the opinions of

scientists with respect to the definition of the death moment of multicellular organisms.

Death as the annihilation of an individual

We have also difficulties in our endeavor to find a good definition of death if we try to adjust this definition to our conception of an individual.

One understands by the word individual mostly a part of the living world which can not be divided into smaller parts without an annihilation of life. According to this opinion, every organism represents an individual, and as only individuals are able to support their life themselves, one concludes that the annihilation of an individual must be accompanied by death. This opinion, in its application to unicellular organisms, leads to the admission that the reproduction of these organisms is accompanied by death. Such an opinion was recently expressed, for instance, by Doms (1921), who considered death as an expiration of the individuality representing a "primary adequate system". A similar but somewhat modified opinion we find in Hartmann's work.

As is known, Weissmann (1892) pointed out that death is characterized by the appearance of a corpse. Disagreeing with Weissmann, Hartmann (1906) affirmed that the death of protozoa is not accompanied by the appearance of a corpse, which is observed only in the case of the death of multicellular organisms. According to Hartmann, death is in all cases a completion and stopping of the individual development. In the case of unicellular organisms, this stopping occurs simultaneously with their reproduction. At the same time, considering the death of single cells in the multicellular organism and necrobiosis, Hartmann emphasized that he understands by the development of an organism not only a constructive, ontogenetic, but also a destructive, degenerative development constituting necrobiosis.

For every modern physiologist it is evident that the life processes do not stop at once after an annihilation of the individual. Many experiments of late years gave striking examples of survival of different animal and human organs. I should like to cite, for instance, the interesting experiments of Kravkov (1922), who succeeded in keeping severed human fingers alive for several months. It proved possible to keep also other severed organs in the living state, as for instance, ears, head and heart (Pissemsky, Kuliabko and others). Moreover, single tissues and cells of animals and plants can be cultivated in artificial solutions, as was shown by Leo Loeb (1908), Harrison (1913), Carrel (1921) and others. Especially interesting are the well known Carrel experiments on the artificial culture of the tissue from the heart of chicken embryo. This tissue is still living at a much greater age than the life span of an intact fowl.

Verworn's hierarchy of individuals

Verworn tried to reconcile the cited observations with the above opinion concerning the necessity of death (that is of stopping of life) after an annihilation of the individual. He proposed to admit that there are individuals of different kinds. Single cells represent, according to Verworn, individuals of the first order. The individuals of the second order are tissues; those of the third order are organs; those of the fourth order are organisms and those of the fifth order are states. The division and transformation of the individual of one kind into the individuals of the other kind is not accompanied by death, but the annihilation of the individual of the first order brings it about.

This compromising opinion seems to me, however, not satisfactory, because, first,

it rejects the usual conception of an individual as an independent organism. Second, it does not set aside all difficulties in the definition of death. Indeed, the moment of death of multicellular organisms does not become more distinct if we adopt Verworn's ideas, namely, we do not know how many individuals of the first and second orders should be dead in order to conclude that the individual of the fourth order, that is the organism, is also dead. Third, it is scarcely correct to assert that life is bound up with the individuality of Verworn's first order, and that the annihilation of this individuality necessarily brings about a stoppage of all physiological phenomena. Indeed, the living matter of the cell consists of protoplasm (cytoplasm), nucleus, chromatophores (plants), fibrils (animals) and other cell organs. It is now proved that protoplasm can be separated from nucleus and remain alive after such separation for a long time. Gerassimoff (1902) showed, for instance, that the cooling of *Spirogyra* at night produces cells which have no nucleus and which, in spite of this, can live for more than two weeks, and perform their physiological functions almost as well as before. The same phenomenon is known for erythrocytes of mammalia, which lose their nucleus soon after their formation and can live sometimes for several weeks. The experiments of Prowazek (1904) and others on the regeneration of protozoa proved also that a piece of the cell containing no nucleus can show sometimes not only metabolism and movement, but also regeneration. A piece of the cell, like surviving organs, can be therefore alive for a long time, and the disappearance of the individuality of the first order does not necessarily bring about death. It does not matter that the separated parts of the cell can live only for a time, because we know complete cells which can live only for a short time (unfertilized generative cells, for instance). The observation of the death of the cell shows also that its different parts do not die simultaneously. The nucleus dies, for instance, sooner than protoplasm, etc. Thus, if we adopt the Verworn idea and consider the experiments cited above, we must call the living protoplasm separated from the nucleus an individual of an order smaller than one. At the same time, protoplasm can be cut into pieces and does not die after such treatment; we must therefore admit the existence of individuals of a still lower order than that of protoplasm. The Verworn row of individuals may become therefore very long.

We see that Verworn's ideas of individuality do not help us to define the moment of death in the case of multicellular any more than of unicellular organisms. Thus, I do not find any reason why we have to renounce the conception of an individual as an independent organism. Only this definition of individuality is comprehensible and should be considered in our discussions concerning the moment of death. But this of course, does not mean that I agree with the definition of death as an annihilation of the individual. On the contrary, I think that such definition of death contradicts our customary opinion that dead organisms do not show any life phenomena, or at least, that these phenomena are restricted to some tissues or cells which are not important for the life of the whole organism.

One can not deny that the cell division of unicellular organisms means an annihilation of one individual and its transformation in to two new individuals. At the same time, the life processes are not at all depressed by the cell division. They continue uninterrupted in both parts of the cell, and in both new individuals. We have therefore no reason to conclude that

any death occurs when a cell divides itself into two cells. On the other hand, death is not always accompanied by an annihilation of the individual, and we could speak of the death of single organs, tissues, or cells and even of the death of living matter, as occurring in a quite healthy organism, which retains its individuality completely. Therefore there is good reason to believe that individuality and death are independent of each other. In order to avoid contradictions we have to define the conception of death independently of any idea of individuality.

Death as the end point of necrobiosis

We may consider first the death of multicellular organisms. I have emphasized above that this death does not occur all at once, but develops gradually, although the development of death or necrobiosis sometimes proceeds very quickly, as for instance in the case of small organisms affected by high temperature or very poisonous substances. It is evident that we can say that death is either this slow or quick process itself or some of its phases. In the first case the word death will have the same meaning as the word necrobiosis, and then we should not be able to speak of "the moment" of death. Furthermore we should have to admit that the organism is dead at the beginning of necrobiosis, that is when most physiological processes are normal.

Hence the only way to find a correct definition of death is to consider the second case, that is to call one phase of necrobiosis death. But what phase could it be?

It is evident that we could not call death the beginning of necrobiosis. Should we call it some intermediate phase of this destructive process? In this case we should have to be prepared to admit that our organism may sometimes revive (after an adequate treatment), that is, we should consider death as a reversible phenomenon, which would be not in accord with our definition of death as an irreversible loss of the property of being alive. Moreover, we should admit in this case that in the dead organism some life processes must be carried on.

The best way to avoid all these contradictions is, according to my opinion, to differentiate between a theoretical and a practical definition of death. The theoretical one should be adjusted to the requirement that dead organisms can not revive or show any physiological phenomena. According to this definition, one could consider only the end phase of necrobiosis as death, that is, the organism could be called dead only if all its living matter is dead.

The practical definition of death should also be in accord with the requirement that dead organisms can not revive, but it may be admitted in this case that some physiological processes can proceed in dead organisms. I should like to suggest, for instance, that physicians may regard a man whose heart has stopped as practically dead if some evidences of a bacterial destruction become distinct (corpse smell, formation of hydrogen sulphide in the mouth, etc.). By this definition of death the burial of living men who are in the state of lethargy would be avoided, and the restoration of men to life after injection of adrenaline into their heart would be easily explained.

Concerning unicellular organisms, their death occurs under the same conditions as the death of multicellular ones. If these conditions become unsuitable for life, as for instance, when some poison is added to the culture solution, or the temperature is too high, the growth becomes slower and finally stops, because the living matter filling the cell gradually dies. Even a

lack of food produces a stoppage of growth and a necrobiosis of all unicellular organisms, because they can not be kept alive forever if their growth has stopped, although some of them take the state of anabiosis under such conditions. The gradual dying of unicellular organisms after the stoppage of growth may be compared with the growing old of the multicellular organisms. In both cases a necrobiosis finally develops which leads to death. If living matter is not renewed, it dies at length.

The difference between unicellular and multicellular organisms is only in the more complex organization of the latter. We have not yet any means to prevent the growing old of higher organisms because we can not induce them to grow and to renew their living matter, and especially to renew the living matter of their nervous system. On the contrary, in the case of unicellular organisms, we can do it by removing poisons from the culture solution and by providing them with food. Under such a condition they do not cease to grow and do not stop cell division, and neither necrobiosis nor death is observed. It does not mean, however, that the unicellular organisms are eternal, because their death does not appear as long as the conditions of life are suitable. In reality, such conditions could not exist on the earth for all individuals: their reproduction is so enormous that all food on the earth is not sufficient to maintain the life of all unicellular organisms. The simplest multicellular organisms, as for instance, radiolaria and polypes, behave like unicellular organisms because, thanks to their small size and the liquid medium in which they live, the removal of poisons and the providing with food can be easily performed.

On the other hand, as was already mentioned, the living matter included in the cell of unicellular organisms dies gradually, that is, shows a necrobiosis. Therefore there is good reason to believe that these organisms do not differ from multicellular organisms in their death, and we shall not make a mistake if we admit that the theoretical definition of the death of unicellular organisms is the same as that of multicellular organisms, that is, death is an end phase of necrobiosis, and the organism can be called dead only if all its living matter is dead. As to the practical definition of death, it is not necessary in this case.

On the basis of the definition of death adopted in this paper we may conclude that the reproduction of unicellular organisms is sometimes but not always accompanied by death, namely when not the whole cell but only its part is transformed into new cells. In this case the remainder of the cell dies, as for instance, in the case of the formation of spores of yeast, etc.

The above definition of death as a death of all living matter of an organism permits us to speak of the death of single organs, tissues and cells of multicellular organisms without any alteration of our present idea of an individual as an independent organism. Indeed, if the living matter of a cell dies completely we may say that the cell dies. If all cells of a certain tissue are dead, we shall call this tissue also dead, and if all tissues of an organ are dead, we shall consider this organ as dead. The theoretical definition of the death of multicellular organisms may therefore be modified as follows: their death is the death of all their cells, tissues and organs.

PART II. THE CAUSES OF THE DEATH OF LIVING MATTER

In the first part of this paper I pointed out that the death of organisms, tissues and cells is first of all the death of living

matter. I proposed to consider the death of every organism as an end phase of necrobiosis, that is, to call an organism dead only if all of its living matter is dead. This was my theoretical definition of death. At the same time I proposed also a practical definition which could be used, for instance, by physicians. In this part of my paper I am going to discuss the causes of the death of living matter.

When one speaks of the cause of the death of living matter one must remember that, although the opinions of medieval naturalists (Galen, Fernelius, Descartes), that some soul or pneuma leaves the body of an organism and causes death, were abandoned long ago, the ideas of vitalists (Bordeu, Barthez, Chaussier and others) or their modifications still are defended by several scientists. Temporarily, I leave these ideas undiscussed, and we shall see afterwards that they are not important for the solution of the problem of death. On the other hand, some theories should be mentioned which consider the cause of death from a purely mechanical or chemical standpoint. To the theories of this kind belongs Pflüger's theory of a living protein. This theory originated in the second half of the nineteenth century soon after the publication of the well known works of Miescher and Hoppe-Seyler, in which the results of the first chemical analysis of living matter were described. This analysis showed that living matter contains much "albumen substance."

Pflüger (1873) supposed that living matter contains "living protein" which differs from dead proteins of our laboratories by a peculiar structure of its molecules. These molecules contain, according to Pflüger, the cyan group, while the molecules of dead protein contain ammonia radical, that is, the amino group; the former is unstable and is very easily transformed into the latter. This transformation, according to Pflüger, causes death.

Verworn (1903), who seemed to adopt Pflüger's theory, substituted the word "biogen" for "living protein," ascribed a great instability to it and compared it with explosive substances. Its decomposition can be produced, according to Verworn, even by stimuli.

The biogen hypothesis was adopted also by Detmer (1882), who supposed that the molecule of this substance is decomposed in the process of metabolism and transformed into a nitrogen compound and a substance which contains no nitrogen. This decomposition, together with the change of the structure of protein molecules, causes death. A like theory of the peculiar chemical structure of living matter was developed also by Allen (1899).

Coagulation and death

On the other hand, Berthold (1886) ascribed death to an irreversible change of an equilibrium and of a distribution of substances in the cell. A coagulation of proteins plays, according to Berthold, an important part in this change. That a congelation and coagulation accompany death was clear also to other scientists, as for instance, to Pfeffer and Arth. Meyer. The application of colloid chemistry to the interpretation of the structures visible in the cells permitted me to regard the changes in living matter accompanying death as a colloid-chemical phenomenon. In my papers devoted to the structure of protoplasm and to the causes of death (1910, 1911, 1912, 1924) I pointed out that living matter, which represents, in active state, a colloidal solution, coagulates simultaneously with death. This coagulation is proved by the congelation of liquid living matter and by the appearance of granules in it. Even in the cases where

living matter apparently looks homogeneous after the fixation (that is, a quick death), an investigation with the ultramicroscope or staining proves the appearance of minute granules in it. The well known structures found sometimes in dead protoplasm (fibrous, net-like, and froth-like structures) are only coagulation structures observed also in coagulated colloids. Moreover, I pointed out that living matter loses its selective permeability after death, that is, it becomes easily permeable not only to lipoid soluble substances but also to all substances soluble in water. This increased permeability of dead protoplasm is comprehensible, because the liquid protoplasm, easily dissolving lipoid soluble substances, becomes transformed into a coagulation jelly consisting of granules mostly visible under the ordinary microscope. Through the pores of this jelly all substances soluble in water penetrate as easily as through the pores of a sponge.

Chemical changes in death

On the other hand, it is well known that in some cases protoplasm partially or completely dissolves in the surrounding water if it is injured or killed, as for instance, in the case of red blood corpuscles, which often show a hemolysis under the conditions which usually bring about death. In the case of colorless cells this process is often called cytolysis. Therefore not only a coagulation but also a solution of the substances composing protoplasm is characteristic of death. This solution is usually explained by assuming that the surface of protoplasm is covered by a thin film (protoplasm skin, *plasmamembran*) which becomes injured to such an extent that it becomes permeable to substances contained in the inner parts of protoplasm. My investigations (1910, 1913, 1923, 1924, 1925, 1926) proved, however, that there is no film on the protoplasm surface which could prevent the diffusion of substances from the cell. The pellicle covering the surface of the red blood corpuscles is easily permeable to hemoglobin, in spite of the fact that the hemoglobin comes out from the cell into the surrounding water only after the corpuscles are injured or killed (1924). At the same time my investigations showed that there is no stroma in living protoplasm on which the diffusing substances could be adsorbed and which could be freed after death. The only correct explanation of solution of protoplasm in the surrounding water after death is therefore the supposition that some chemical changes, which free the diffusing substances, occur in protoplasm simultaneously with death.

Indeed, there is some evidence that death is accompanied by a chemical decomposition of substances forming protoplasm. In one of my previous papers (1928) I cited this evidence. It is proved that protein substances and lipoids are not free in living protoplasm, but become free (sometimes only partially) after death, and that, at the same time, lipoids are maintained in living protoplasm by protein substances. I stated therefore that protein substances and some lipoids are probably combined with each other in living matter, but their compounds, which I called principal compounds, are very unstable and decompose under the influence of different harmful effects.

Detmer's opinion, that "biogen" decomposes to a nitrogen compound and a nitrogen free substance, is therefore near my opinion. At the same time, Pflüger's idea of the different chemical structures of the protein molecule in living and dead protoplasm is wrong, because, according to Emil Fischer's investigation of proteins, their molecule contains no cyan group.

The substance which would contain this group instead of the amino group could not be called protein.

Thermic effects of death

It was mentioned that the principal compounds forming living matter are very unstable and decompose even under the influence of a relatively slight mechanical effect. As the decomposition of very unstable chemical compounds is usually accompanied by the production of heat, we may expect the decomposition of the principal compounds of living matter and death to represent an exothermic process. Indeed, my experiments (1929) have shown that the death of yeast cells is accompanied by the production of heat. The thermic effect observed in this case is about 2 gm. calories per gram of dry substance, and is therefore small as compared with the heat produced in most exothermic chemical reactions (calculated per gram of the decomposing substance). On the contrary, if we try to determine the heat produced by the decomposition of one molecule of the principal compounds composing living matter we will find that this heat is very great, because the molecular weight of these compounds is evidently greater than that of protein substances. Among these substances hemoglobin is one the molecular weight of which is determined. It is at least 16,700. The heat produced by the decomposition of one molecule of the principal compounds of living matter is therefore greater than 40 kilo calories, while the heat produced by the decomposition of one molecule of nitrogen chloride, that is, of one of the most unstable explosive substances, is only 38 kilo calories, etc. In my recently published experiments on red blood corpuscles I found the same thermic effect produced by their death or hemolysis (1930). This explains the great instability of the principal compounds of living matter, which could be compared only with that of most unstable explosive substances.

There is therefore good reason to believe that the death of living matter is accompanied by a decomposition of some very unstable chemical compounds, it being very probable that these compounds (principal compounds) are formed by protein substances and lipoids. On the other hand, there is some evidence that these compounds play an important part in the formation of the dispersion medium of protoplasm and of living matter in general (see my paper cited above). Hence, the decomposition of the principal compounds brings about a destruction of this medium. Indeed, I found that in all investigated cases this medium is replaced by water, which fills the channels of the coagulation jelly of protoplasm after its death. The destruction of the dispersion medium of protoplasm after death easily explains the congelation of liquid protoplasm. Indeed the products of the decomposition of the principal compounds forming this medium are mostly insoluble in water, and are precipitated in the form of granules. These granules join the granules of the colloidal substances which have been dissolved in living protoplasm and become precipitated after their dispersion medium was destroyed.

On the contrary, if the products of the decomposition of the principal compounds forming the dispersion medium and the substances dissolved colloidally in protoplasm are soluble in water, or at least, if some of these substances are soluble in it, no coagulation, or only a partial coagulation of protoplasm is observed. At the same time, the whole mass or a part of protoplasm dissolves in the surrounding water. But even in these cases it is possible to bring about a marked coagulation of protoplasm if the cells are killed by

poisons which form insoluble compounds with the products of the decomposition of the principal compounds and with colloidal substances dissolved in protoplasm. This occurs, for instance, after the action of corrosive sublimate, which does not produce any hemolysis of red blood corpuscles or cytolysis of colorless cells, if taken in great concentrations.

If the dispersion medium of a colloidal system is destroyed, the whole system is evidently destroyed. As the chemical agents of the cell and the materials which are used in metabolism are dissolved or colloidally dispersed in liquid living matter, they may dissolve after death in the surrounding water or be adsorbed by the coagulated substances. They may be also precipitated in non-dissolved state after death. In every case they become lost for the cell, because only chemical reactions between substances dissolved in living matter are important for its metabolism. Hence, we may expect the chemical reactions forming metabolism to stop or to be modified to such extent that they could not be of any use for the cell. But if metabolism stops, the other life processes must also stop, because they are closely connected with metabolism. It is well known that growth, movement and reproduction are most sensitive to all changes of metabolism. They are sometimes especially sensitive to the changes in the oxygen metabolism, and stop at once if it ceases.

Is death the result of chemical and physical processes?

We arrive therefore at the conclusion that if the principal compounds of living matter become decomposed, life must stop. We now may ask whether this decomposition and the destruction of the whole colloidal system of living matter result from chemical and physical processes in it, or are produced by a withdrawal of some kind of soul or by an annihilation of a living force, etc.

In order to answer this question we may keep in mind that the principal chemical, compounds of living matter which are present in its dispersion medium are so unstable as to be compared to explosive substances. On the other hand, it is well known that explosive substances can be decomposed by mechanical effects, by high temperature, sometimes by very strong light, and by all chemicals which act on different parts of their molecule. The destruction of the dispersion medium and of the whole colloidal system of living matter is produced by the same factors, that is, by mechanical effects, by high temperature, sometimes by very strong light, and by all chemicals which react with their component parts, that is, with protein substances and lipoids. Protein substances are chemically altered by acids, alkalies, salts of heavy metals, high temperature and strong light. All these agents produce death. All agents which alter lipoids chemically (for instance, saponin and iodine) also produce death.

It is therefore proved that all agents which produce a chemical alteration of the component parts of the principal compounds of living matter bring about their decomposition and the destruction of the whole colloidal system of protoplasm. Thus it is evident that in the case of the action of the above mentioned agents on the cell, death is brought about by the chemical alteration of the component parts of living matter, and is therefore independent of a soul or other unknown mystic phenomenon. The chemical alteration produced by the mentioned agents is quite sufficient to produce death. Hence, if somebody thinks that the soul or any vital force, etc., cause life he must admit that both leave the cell in this case because life

can be no more maintained in dead protoplasm. First living matter dies, then soul, etc., leave the cell.

If we consider now the other cases of death, we shall come to the conclusion that they are produced mostly by poisons which are accumulated in tissues or in the surrounding medium, as, for instance, the death produced by many pathogenic parasites and in general by toxins, by suffocation and by saturation of the surrounding medium with decomposition products formed in metabolism. It is certainly unknown how all these poisons bring about the decomposition of the principal compounds of living matter, but it would be quite incomprehensible if they act first on some mystic part of living matter in this case. It is more probable that in all cases poisons act on the component parts of the principal compounds chemically or produce a coagulation in protoplasm which acts mechanically on these compounds and causes them to decompose.

Natural death

Concerning so-called natural death, that is, death which occurs apparently without any change in the surrounding medium, or in the life and state of an organism, it is caused sometimes by a gradual poisoning of the cells, sometimes by a spontaneous decomposition of the principal compounds. A spontaneous slow decomposition is known for all unstable substances, as, for instance, for explosives. In the case of unicellular organisms natural death occurs if life processes are caused to stop by a very low temperature, drought, or want of food. The state in which the organism is in this case is known under the name of anabiosis. The principal compounds of living matter, being very unstable, must slowly decompose if their supply in the cell is not renewed by metabolism. Only in the case of very low temperature at which all chemical reactions stop, the decomposition of the principal compounds could almost stop too, and living matter could remain alive for a very long time even if it were not renewed.

In the case of higher organisms natural death is known to occur as a result of a gradual death of the nervous system. The causes of this death probably lie in a poisoning of the nervous cells by the decomposition products of metabolism and also in a spontaneous decomposition of the principal compounds of living matter.

It is therefore very probable that in all cases death is caused by the decomposition of these compounds and by the succeeding destruction of the whole colloidal system of living matter. If one should like to suppose that every organism possesses a soul, living forces or other mystic part, one must admit that they leave the body after death because of the impossibility of life in it, but they do not produce death themselves, by leaving the body.

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THE "CONCEPT OF ORGANISM" AND THE RELATION BETWEEN EMBRYOLOGY AND GENETICS

PART III

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PART II of this article (10) was devoted to a study of some of the postulates, assumptions, and order-systems upon which all inference in genetical and embryological science depends. It was pointed out that the whole biological realm could be regarded as exemplifying what is here called "hierarchical order." This means, roughly, that in this realm there are entities constituting systems with respect to relations having the formal properties of the relation which has been symbolized by R_H. The fundamental relation upon which all the rest depends is that between a given cell and the cell of which it is one of the immediate division products. It was symbolized by $R_H(d)$. Cells, therefore, are the entities constituting the *field* of this relation, and are divisible into three groups: (1) those belonging *only* to the *domain* of $R_H(d)$ (hence cells which do not divide); (2) those belonging *only* to the *converse domain* of $R_H(d)$ (hence zygotes which divide); and (3) those belonging to *both* the domain *and* the converse domain of $R_H(d)$ (i.e. all cells, except those which do not divide, and zygotes). Gametes belong to the first group but are distinguished from the rest by the fact that they also belong to and exhaust the converse domain of the second fundamental relation, namely, that between a zygote and each of the two gametes from which it is formed by fusion (R_f). Thus three kinds of cells are distinguished in the above way: (i) zygotes (symbolized by o), i.e. cells belonging to the domain of R_f and the converse domain of $R_H(d)$; (ii) gametes (g), which belong to the converse domain of R_f and to the domain of $R_H(d)$; and (iii) all other cells, namely those which do not belong to the field of R_f. These will be called "c-cells." The only cells which do not satisfy the above definitions are gametes which do not unite and zygotes which do not divide, but as neither of these types are important from the present standpoint they can be neglected. Zygotes which do not divide are the only cells which do not belong to the field of $R_H(d)$.

Every cell which belongs to the field of $R_H(d)$ is a member of a *system* of cells standing in hierarchical order, and here called a division hierarchy (dW), the ordering relation being $R_H(d)$. In Part II it was shown how the powers of this relation determine certain important classes of cells. One of these was defined as "the class of cells consisting of all the contemporary descendants of a given zygote at moment m." In Metazoa these cells are so related to one another as to constitute *components* of one organism. Together they constitute a level (the cell-level) in a *spatial* hierarhy (sW) in which there may be higher levels. Components (as contrasted with constituents) are of three principal kinds: (1) cellular

components of the first (P'), second (P''), or third (P'''), etc., orders; (2) "non-cellular" components, i.e. cells which are components (either c- or g-cells); and (3) cell-components of the first (p'), second (p''), or third (p'''), etc., orders, according to the degree to which analysis is carried. The spatial whole (sW) represents the entity "W" of the abstract definition of hierarchical order, and the corresponding relation, $R_H(s)$, is the relation of a given component of level L_n to the component of level L_{n-1} *of which* it is a component. Different spatial hierarchies (which in Metazoa may also be called "temporal slices" or "temporal parts" of ideally small temporal extent) of a given division hierarchy will be distinguished by subscript letters, thus: sW_n, sW_m, sW_x, etc. The sW_n (say) of dW_1 will be written "sW_n' dW_1" (the inverted comma being read "of") to distinguish it from the *corresponding* slice of another similar division hierarchy dW_2, which will accordingly be written "sW_n' dW_2."

The genetic hierarchical relation, $R_H(g)$, was defined as the relative product of the converse of $R_H^p(d)$ and the converse of R_f, when the converse domain of $R_H^p(d)$ is limited to zygotes, and its domain is limited to gametes. In this way we obtain a relation between zygotes, which thus constitute the field of $R_H(g)$. A genetic hierarchy with respect to a given zygote o_1 was then defined as the class of zygotes consisting of o_1 and all zygotes standing in $R_H^p(g)$ to o_1. It was also shown how powers of this relation determine classes of zygotes which are precisely analogous to the corresponding classes determined by powers of the other hierarchical relations. The resemblances and differences between this and the other hierarchies were also pointed out. Having now sufficiently established these three exemplifications of the hierarchical relation (i.e., the ordering relation in a system of entities in hierarchical order) we can abandon the cumberous symbols $R_H(d)$, $R_H(s)$, and $R_H(g)$, and substitute R_d, R_s and R_g respectively, with corresponding alterations for the powers of these relations.

Some explanation was given in Part II of the different kinds of *difference* which have to be considered in a causal analysis. We distinguished *manifest* difference (D_m), which is immediately observed, from *intrinsic* difference (D_i), which is assumed in accordance with the causal postulate. Intrinsic difference was also said to be of two kinds: either (1) *relational* difference (D_r), when the difference between two entities is regarded as being dependent upon a difference between two *other* entities in their (approximately) simultaneous environment; and (2) *specific* difference (D_s), for a difference between two entities which is not regarded as being dependent upon a difference between two other entities. The adjective "specific" will here be used exclusively in this sense, not in any special biological sense. It will be seen below that we have to discriminate between a number of quite distinct kinds of specific difference in biology which are frequently confused.

A few words must be added about "identity" and "non-difference." When we say "x is identical with y," where x and y are organisms or parts of organisms, we may mean one of two things. (1) We may mean merely that "x" and "y" are two different symbols for one and the same entity. For this meaning I use the sign "=." (2) But more commonly we mean that x and y are two place-dates (see Part II, p. 444) with (ideally) the same intrinsic pattern, e.g. two (theoretically) "identical" twins. For this relation I shall use the symbol "I," and shall

refer to it as "non-difference." Subscripts corresponding to D_i, D_s, D_m will also be used. Thus "$(o_1)I_s(o_2)$" will mean "the zygote o_1 is not specifically different from the zygote o_2." We always assume (given "normal" conditions of perception) that D_m implies D_i, but of course we cannot say that I_m implies I_i (cf. Part II, p. 446).

Something must be said about the theory of comparison, although justice cannot be done to this difficult subject in a short space. We must to a very large extent follow the usual practice of relying on the reader's intuition to bridge over difficulties which have not yet been subjected to logical analysis. In the first place what is compared is always the manifest character (in the widest sense) correlated with the two entities. In biology we compare the manifest pattern correlated with one spatial whole (sW), or with a spatial part of that whole, with the manifest pattern correlated with another spatial whole, or with a *corresponding* spatial part of that other whole. There are then two distinct cases: (1) when the spatial wholes concerned belong to *different* division hierarchies (taxonomic biology), and (2) when they belong to the *same* division hierarchy (embryology). In the first case, not only must the spatial parts compared be "corresponding" or "homologous" parts (no one who wanted to compare two dogs would compare the tail of one with the head of the other), but the temporal parts or slices (sW) of the two division hierarchies compared must also be "corresponding" or "temporally homologous" parts. No one who wished to compare two different gramophone records of the same symphony would compare the first movement of one with the second movement of the other. He would compare "corresponding" movements. We should not say that one performance of a symphony gradually "comes to resemble" another performance of the same symphony. But we often find in biological literature references to "that wonderful process" by which a fertilized ovum "comes to resemble" its parents. This is an instance of the way in which "problems" have been created by bad terminology and inadequate analysis. For it is evident that two division hierarchies which are related as parent to offspring resemble one another *throughout*, like the two performances of the same symphony. Zygote resembles zygote, gastrula resembles gastrula, and so on. Each successive slice as it is realized resembles the *corresponding* slice of the parental series.

We have the general term *color* to cover red and blue and green, etc., we have the general term *length* to embrace all the particular lengths, and similarly the term *weight* for the various particular weights. But there seems to be no term in general use for color *and* length *and* weight *and* . . . etc., so that it will be useful to use the word *determinable* for this purpose (see Johnson (5)). We can then say that in genetic experiments what is compared is the particular determinate character d_1 under the determinable δ of the spatial part μ of the slice sW_n of the division hierarchy dW_1, with the particular determinate d_2 under the determinable δ of the spatial part μ of the slice sW_n of the division hierarchy dW_2. We might, for example, be comparing a particular shade of gray under the determinable color of the skin of the adult rabbit *A*, with the particular shade of brown under the determinable color of the skin of the adult rabbit *B*. In each case the determinable, the spatial part, and the temporal slice must be "the same" although displayed in different division hierarchies. In embryological investigations the procedure is different, but the difference can only be made clear as we proceed.

II

Our next task is to make clear the different sorts of specific difference which are involved in biology. We must first state the basic theorems upon which all reasoning in embryology and genetics rests. In what follows "*dW*" will always mean a division hierarchy throughout its temporal extent, from the zygote up to the latest slice considered, and "*EdW*" will mean the environment of that division hierarchy throughout its extent up to the latest slice considered. For brevity we can write "sW_n ' dW_1" for "the particular slice *n* of the division hierarchy 1," the inverted comma being read "of." Subscripts are merely distinguishing tags without any ordinal significance unless the contrary is stated. It must be remembered that we are not here concerned with the question how these theorems are applied in practice, but with what is ideally involved if *exact inferences* in regard to these topics are to be possible. We do not need to repeat the whole rigmarole about determinate characters, determinables, etc., but, taking this as now understood, we can simply state that two corresponding slices of two different division hierarchies are or are not *manifestly* different. Our first theorem can therefore be stated as follows:

(1) If

$$(sW_n \text{ ' } dW_1)\ D_m\ (sW_n \text{ ' } dW_2).$$

then (by the causal postulate and definition of D_i)

$$(sW_n \text{ ' } dW_1)\ D_i\ (sW_n \text{ ' } dW_2).$$

Hence (by definition of D_s):

(2) If

$$(sW_n \text{ ' } dW_1)\ D_i\ (sW_n \text{ ' } dW_2)$$

and

$$(EsW_n \text{ ' } dW_1)\ I\ (EsW_n \text{ ' } dW_2)$$

then

$$(sW_n \text{ ' } dW_1)\ D_s\ (sW_n \text{ ' } dW_2).$$

This, then, is one sort of specific difference, a specific difference, namely, between two corresponding slices of two division hierarchies, which slices have the same environment. But suppose the environments of the two division hierarchies were non-different *throughout*, i.e., from the zygote *o* up to sW_n, then if sW_x is *any* slice prior to sW_n we shall have the following theorem:

(3) If

$$(sW_n \text{ ' } dW_1)\ D_s\ (sW_n \text{ ' } dW_2)$$

and

$$(EdW_1)\ I\ (EdW_2)$$

then

$$(sW_x \text{ ' } dW_1)\ D_s\ (sW_x \text{ ' } dW_2).$$

Moreover, since the zygote is a slice of a division hierarchy (the "first" slice) it is a value of sW_x, and we have as a corollary:

(4) If

$$(sW_n \text{ ' } dW_1)\ D_s\ (sW_n \text{ ' } dW_2)$$

and

$$(EdW_1)\ I\ (EdW_2)$$

then

$$(odW_1)\ D_s\ (odW_2).$$

These two theorems are obviously ordinarily assumed in genetic interpretations. If two eggs are incubated in the same incubator and if the birds that hatch out are manifestly different, few geneticists would hesitate to assume that the zygotes had been specifically different. It might not even be recognized as an assumption, but it is evident that these theorems are simply consequences of the a priori causal postulate.

Now it is plain that in theorems (3) and (4) we have introduced much more than was involved in "specific difference" as we find it in theorem (2). In the latter case we have a difference between two spatial hierarchies which involved no

explicit reference either to the earlier slices of the division hierarchies to which they belong, or to the environments of the latter. Theorems (3) and (4), on the other hand, clearly state that there is a specific difference which is pervasive *throughout* the division hierarchies up to sW_n, so long as the environments have been non-different throughout. It will not, therefore, be surprising to find that the interpretation of "specific difference" in the two cases is different. But before this is discussed we must consider some embryological theorems. Let sW_n and sW_m be two slices of the *same* dW, sW_m being later than sW_n, then:

(5) If

$$(sW_n)\ D_m\ (sW_m)$$

then (by the causal postulate)

$$(sW_n)\ D_i\ (sW_m).$$

(6) If

$$(sW_n)\ D_i\ (sW_m)$$

and EdW has been constant or uniform throughout up to sW_m, then

$$(sW_n)\ D_s\ (sW_m).$$

This evidently follows from the definition of D_s, the causal postulate, and the hypothesis that EdW has been uniform (a condition which is presumably closely approximated to in amniotes for considerable periods). Thus theorem (6) *again* introduces specific difference, and we now have to consider its interpretation in relation to the others.

When we say that two corresponding slices of two *different* division hierarchies are specifically different in the sense of theorems (3) and (4) we usually mean that they belong to different races, or species, or genera, etc., but when we say that two slices of the *same* hierarchy are specifically different in the sense of theorem (6) do we mean that they are specifically different in the *same* sense? Do we suppose, for example, that a human zygote is specifically different from an adult man in the *same sense* in which a human zygote would be regarded as specifically different from an amoeba? Do we suppose that a human zygote belongs to a different race, species, or genus, etc., from an adult man? I take it that we do not and that, in consequence, specific difference means something quite different in the two cases. Even those people (if there are any left) who regard all zygotes as amoebae with their pseudopodia on the first rung of the ladder by which each "climbs up its own ancestral tree," will be compelled to make this distinction. They will be compelled, that is to say, if they admit the causal postulate, to assume some difference between the several alleged "amoebae" in accordance with which they climb up *different* trees.

There is another way in which we might bring out this point. Let odW_1 and sW_n be two slices of dW_1, namely the zygote and some later slice, such that $(odW_1)\ D_s(sW_n\text{`}dW_1)$ in the sense of theorem (6). Let $odW_2$ and $sW_n\text{`}\ dW_2$ be corresponding slices of dW_2, such that $(odW_2)D_s(sW_n\text{`}\ dW_2)$ in the same sense. Let $dW_1$ and $dW_2$ both have the same environment up to $sW_n$, and be specifically different in the sense of theorem (3). Now since $(odW_1)D_s(odW_2)$ we also have $(sW_n\text{`}dW_1)\ D_s(sW_n\text{`}dW_2)$. For since $(odW_1)D_s(odW_2)$ and $(EdW_1)I(EdW_2)$ we cannot have $(sW_n\ \text{`}dW_1)I(sW_n\text{`}dW_2)$. Because if $(sW_n\text{`}dW_1)\ I(sW_n\text{`}dW_2)$ when $(odW_1)D_s(odW_2)$, then (by the causal postulate) either: $(sW_n\text{`}\ dW_1)D_s(odW_1)$, or $(sW_n\text{`}dW_2)D_s(odW_2)$. But embryologically we *do* have $(sW_n\text{`}dW_1)D_s(odW_1)$, etc. Hence D_s means something different in embryology from what it does in genetics. In other words

the specific difference between a human zygote and a monkey zygote is a *different sort* of specific difference from the difference between a human zygote and an adult man, although both are specific differences in the sense defined in Part II. (These two sorts of difference seem to have been confused in the doctrine of recapitulation). We must therefore seek for an interpretation of the two kinds of specific difference which will make this distinction clear, and we require to use a corresponding difference of language. I shall call the difference between two specifically different division hierarchies a *genetic* difference (D_g), and the difference between two specifically different slices of the *same* hierarchy will be called a *developmental* difference (D_d).

Now since, as was pointed out above, the specific difference of theorem (3) is one which is pervasive *throughout* the two division hierarchies, so long as their environments are non-different, its interpretation can, it seems, only be sought in something which is common to *all* slices. And one thing which we can say about every slice is (by definition, see Part II) that it consists of a class of cells (in the case of the zygote-class this is a class with only one member). Consequently the hypothesis suggests itself that when we say that two division hierarchies are genetically different, or specifically different in the sense of theorem (3), we are really making an assertion about the *kind of cells* occurring in the two hierarchies, i.e. we are saying that all the cells of the one hierarchy belong to one kind of cell, and all the cells of the other hierarchy belong to another and different kind of cell, or that any cell of the one hierarchy is specifically different from any cell of the other, in the same sense in which the zygote of the one is (by hypothesis or inference as in theorem (4)) specifically different from the zygote of the other. And since every cell in a division hierarchy stands in a relation which is some power of R_d to the zygote of that hierarchy, this is equivalent to saying that if c_1 and c_2 are any two c-cells (belonging to dW_1 and dW_2 respectively, odW_1 and odW_2 being the two corresponding zygotes) then, if $(odW_1)D_s(odW_2)$, and if $(c_1)R_d{}^p(odW_1)$ and $(c_2)R_d{}^p(odW_2)$, then $(c_1)D_s(c_2)$. But if, on the other hand, c_1 and c_2 are such that $(c_1, c_2)R_d{}^p(odW_1)$, i.e. both belong to the *same* division hierarchy, then $(c_1)I_s(c_2)$. Let this be provisionally adopted as an interpretation of D_s in the sense of theorem (3). It will also serve as a definition of genetic difference as that expression is here used, i.e. (setting out the definition in full): "$(k_1)D_g(k_2)$" is to mean: "k_1 and k_2 are two cells such that $(k_1)R_d{}^p(odW_1)$ and $(k_2)R_d{}^p(odW_2)$, and sW_n'dW_1 and sW_n' dW_2 are two corresponding slices of dW_1 and dW_2 respectively and such that $(sW_n$'$dW_1)D_m(sW_n$'$dW_2)$ and $(EdW_1)\ I\ (EdW_2)$."

We can leave any further discussion of the above hypothesis for the present in order to turn to the interpretation of the second kind of specific difference, i.e. developmental difference, the definition of which may be stated as follows: If sW_n and sW_m are two slices of the same division hierarchy, sW_n being earlier than sW_m, then "$(sW_n)D_d(sW_m)$" is to mean: "$(sW_n)D_s(sW_m)$ and EdW has been uniform up to sW_m." For the interpretation of this kind of specific difference we have to recall what was said in Part II about the interpretation of specific difference between two organized entities. It was there pointed out that we ordinarily assume that if two such entities are specifically different this is because they differ: (1) in the *number* of their parts, and/or (2) in the *relations* in which those parts stand to one another, and/or (3) in the

kinds of parts into which they are analysable. Applying this postulate (which we shall refer to as the Postulate of Analytical Interpretation) to the case of $D_d(sW_n, sW_m)$ we can proceed as follows: sW_n and sW_m are both (by definition of sW) analysable into cells, consequently they will differ in (1) the *number* of cells, and/or (2) the *relations* between the cells, and/or (3) the *kinds* of cells, into which they are analysable. Now, in the course of development, by the mere continuation of division (or spatial repetition of the cell-pattern), we have an increase in the *number* of the cells, and this same process, coupled with movements of cells, or of cellular components, will yield differences in the *relations* in which the cells stand to one another (in the spatial hierarchy), whilst, in later slices especially, we soon begin to find *manifest* differences between the cells, from which we infer (by the causal postulate) that such cells are *intrinsically* different, and if these differences are not purely relational, we shall have cells within the hierarchy which are *specifically* different, i.e. different *kinds* of cells. Thus all the three possibilities offered by the Postulate of Analytical Interpretation appear, in general, to be realized in developmental difference. But the admission that there may be specifically different cells in the *same* division hierarchy appears to conflict with our previous assumption that if c_1 and c_2 are any two c-cells of a given dW then $(c_1)I_s(c_2)$. But this we called a genetic difference or non-difference, and it may be possible for two cells in the same dW to be specifically non-different in *this* sense, and yet be specifically different in some *other* sense. It seems, then, that we shall have to distinguish yet another kind of specific difference, and again a difference between *cells*. That is to say: there may be two *different sorts of sorts* of cells. And, if you are classifying cells according to their genetic sort (as above defined) then all the c-cells of a given dW may be of the same sort, or genetically non-different, whereas, if you are classifying them according to some *other* mode of classification the cells of a given dW may *not* be all of the same sort. If this were the case there would be nothing contradictory in our procedure.

It was pointed out in Part I (9) p. 12, that there are two ways in which cells which are specifically different may arise in a division hierarchy (although the expression "specifically different" was not used in Part I). These two ways are (1) differentiating division, and (2) histological elaboration. If a cell divides in such a way that, in consequence of the division, the cells are specifically different (as appears to be the case in the "mosaic eggs") we have differentiating division. But if the two resulting cells are not specifically different but only *become* so *after* division, in consequence of their different organic relations in the spatial hierarchies, then the two cells will be said to have undergone histological elaboration. For example, numerous experiments have now shown that the sort of histological elaboration a cell or its cell-descendants will undergo depends, in a given division hierarchy, on its relations to certain other cells, or cellular components. Maximow (6) gives an example from hematology. He believes that the large lymphocyte-like cells of both the lymph nodes and the bone-marrow are not specifically different, but that the differences manifest in their cell-descendents are a consequence of their different relations in lymph nodes and bone-marrow respectively. He writes:

> Using the method of tissue culture, Maximow attempted artificially to change the external conditions for the lymphocytes by explanting them into a medium which would as nearly as possible correspond to the medium normally surrounding the lymphoid

stem cells in the myeloid tissue and might induce them to display their latent granulopoietic potencies. In fragments of lymphoid tissue, developing in vitro in a mixture of blood plasma and bone marrow extract, he observed proliferation of large lymphocytes with differentiation into special and eosinophilic myelocytes and megacaryocytes.

In this way, then, we may have specifically different cells in the same division hierarchy although they may not be specifically different in the sense of being *genetically* different. We therefore require a name for this new kind of specific difference between cells belonging to the same division hierarchy. I shall call it *histological* difference (D_h), but it is not to be understood in a narrow, static or "anatomical" sense, but in a wide sense to include dynamic or "physiological" differences as well. For the present we may define this difference as follows: If c_1 and c_2 are two cells such that $(c_1)D_s(c_2)$, and o is a zygote such that $(c_1,c_2)R_d{}^p(o)$, then $(c_1)D_h(c_2)$.

Another convenient way of expressing this difference between two sorts of sorts of cells is by saying that cells which are *not* genetically different belong to the same "*G*-type," and cells which are not histologically different belong to the same "*H*-type." And our assumption so far is (provisionally) that two *c*-cells belonging to the same division hierarchy are of the same *G*-type, but need not be of the same *H*-type. If they are manifestly different they will probably be specifically different, and so of different *H*-type, e.g. a muscle-cell and a nerve-cell of the same division hierarchy.

It is clear from the above that genetics is primarily concerned with the study of cells from the standpoint of their *G*-type, not with "characters." The latter are only an index of whether the cells of two division hierarchies, having the same environment, are or are not specifically (and hence genetically) different. The aim of the geneticist is to avoid the necessity of referring to the environment by keeping it the same for both of the division hierarchies compared. But of course this does not mean that the environment is "of no importance." If it were so no such precaution would be necessary. Embryology, on the other hand, is concerned with the process of passage from one *sW* to another in the same *dW*, and, in so far as it can occur in a uniform environment, this is an *immanent* process, and this again (as here understood) does not mean that the environment can be ignored, but only that *if* the process occurs in a temporally uniform environment, then the changes in the former cannot be interpreted as resulting from a one-one correlation with changes in the latter, since, by hypothesis, there are none.

There is still another case of "specific difference" not yet dealt with—that, namely, in theorem (2), in which we have $(sW_n{}'\ dW_1)D_s(sW_n{}'\ dW_2)$ when $(EsW_n{}'\ dW_1)I(EsW_n{}'\ dW_2)$. I shall say, in this case, that the two corresponding slices of the two division hierarchies are *taxonomically* different (D_t). This term may not be free from objections, but so long as the exact sense in which it is here used is remembered, no confusion need result from its use, and it seems better to use this term than to invent a totally new one. Taxonomic difference in this sense, then, is simply an actual specific difference between corresponding slices of *two* division hierarchies, and is thus distinguished from developmental difference, which is an actual specific difference between two slices of the *same dW*. It is to be interpreted in the same way as the latter by an appeal to the Postulate of Analytical Interpretation. Thus, if two such slices are taxonomically different they will differ (1) in the *number* of cells into which they are analysable,

and/or (2) in the *relations* in which the cells stand to one another, and/or (3) in the *kinds* of cells into which they are analysable. In actual practice, of course, we do not usually possess the requisite data to enable us to express taxonomic difference in this way. We express it more often in terms of the determinate characters which the two slices exhibit under various determinables, or in terms of the possession or lack of various cellular components or constituents (manifest difference), although theoretically these should ultimately be expressible in terms of (1), (2), or (3) above (or combinations of them), if we adopt the causal postulate and that of analytical interpretation. In regard to (3) we can now see that, in view of our assumption that there are two kinds of kinds of cells, there will be two possibilities with respect to *G*-type. The cells of the two spatial hierarchies compared will either belong to the *same* or to two *different* *G*-types. If the former is the case then, assuming the casual postulate, it will follow that the taxonomic difference will be wholly the result of environmental differences (i.e. differences in the environments of dW_1 and dW_2) even although the environments of the *actual slices compared* are (by hypothesis) not different. We could say that in that case the two taxonomically different spatial hierarchies are also *environmentally* different. Similarly, if we knew that the zygotes of the two division hierarchies concerned were of *different* *G*-type (i.e., genetically different), and if their environments had been non-different throughout, then we could say that the taxonomic difference between the two corresponding slices compared was wholly the result of the genetic difference between their cells. Thus two taxonomically different spatial hierarchies may be (1) wholly environmentally different and not genetically different; (2) genetically different but not environmentally different, or (3) "mixed" i.e. belonging to division hierarchies which have had different environments and whose cells are of different *G*-type. In actual practice the last possibility will, presumably, be that most often realized, although it is the aim of experiment to have either (1) or (2), and not (3), realized. But it must always be remembered that our fundamental theorems are a priori and "ideal" in the same sense in which Newton's first law of motion is a priori and "ideal." They can be neither proved nor disproved by an appeal to experiment. They serve rather to determine the form and limits of the "logical space" in which we propose to let our thoughts move in regard to these matters. They are adopted because of their success in guiding experiment and in systematizing our data.

The results of the foregoing analysis of specific difference can be summarized in a table which shows what entities constitute the field of these relations of difference, and thus determine their range of significant application.

Relation	*Field*
D_g	Specifically different cells of different division hierarchies.
D_h	Specifically different cells of the same division hierarchy.
D_d	Specifically different slices of the same division hierarchy.
D_t	Specifically different corresponding slices of different division hierarchies.

Note: Although "D_g" is strictly a difference between *cells* it is also convenient to speak of genetically different dW's, sW's, or cell-cones. This will mean, therefore, that *any* *c*-cell of the one dW (sW or cell-cone) is genetically different from *any* *c*-cell of the other dW (sW or cell-cone).

We can now pass on to consider briefly how the above analysis can be applied to the clarification of the problems involved in

(1) The interpretation of the developmental process.
(2) The relation between *G*-type difference and taxonomic difference.
(3) The interpretation of histological difference.
(4) The interpretation of genetic difference.
(5) The distribution of *G*-type differences among the members of a genetic hierarchy.

III

We can begin with the first of these questions. The interpretation of development requires an analysis of the process in which developmentally different spatial hierarchies in the same division hierarchy are successively realized. Our definitions of differentiating division and histological elaboration (see above) provide us with four theoretically possible types of division hierarchy according to the occurrence or non-occurrence in them of either or both of these processes:

(1) Division hierarchies in which neither differentiating division, nor histological elaboration occurs.
(2) Division hierarchies in which differentiating division, but no histological elaboration, occurs.
(3) Division hierarchies in which histological elaboration, but no differentiating division, occurs.
(4) Division hierarchies in which *both* differentiating division *and* histological elaboration occur.

If type (1) were realized in nature it is plain that two slices of such a division hierarchy could not differ from one another in the *kinds* of their cells, apart from differences arising merely from different environmental contingencies. They would be "equipotential systems" in Driesch's sense. Such a state of affairs conflicts so clearly with what we observe in the development of most organisms that we seem to have but three alternatives: (i) to deny that type (1) occurs in nature, (ii) to suppose that it occurs and then either (a) to reject the causal postulate and that of analytical interpretation, or (b) to adopt Driesch's view of development, which is equivalent to retaining the causal postulate whilst rejecting the postulate of analytical interpretation. And since it is a question of choice of a priori postulates it is clearly useless to suppose that such questions can be settled by an appeal to experiments, as Driesch does. Unless, then, we are prepared to revise our usual postulates in natural science, we are driven to assume that division hierarchies of type (1) do not occur in nature. I said in Part I (p. 21) that it is "important to know whether there *really are* equipotential systems in the organic realm," but it seems clear to me now that, *so far as whole organisms are concerned*, we are driven to assume that there are no such things, so long as we adhere to our present postulates.

If type (2) occurred in nature we should have to say that all differences between different slices of the same division hierarchy were a consequence of differentiating divisions, (apart, of course, from differences resulting from environmental changes). Such cases may occur in nature, but transplantation experiments clearly exclude the possibility of regarding all division hierarchies as of this type. Moreover, it is difficult to see how, if all histological elaboration is rigorously excluded, it would be possible to have gametes in later slices of the hierarchy, although the difficulty is not in principle greater in regard to gametes than in regard to muscle or nerve cells, except that gametes are required to have, in general, the same properties as those which went to the formation of the zygote from which the hierarchy was generated.

If type (3) occurred in nature it would

again be extremely difficult to interpret development with out present postulates. We should then have to suppose that *all* differences between cells and cellular components (except differences in number of cells in different slices) were, in origin, *purely* relational. This would mean either that they resulted from purely contingent environmental differences between the cells, or we should have to suppose that they can arise in precisely similar cells simply in consequence of the different relations in which they come to stand in the course of cleavage. There are plenty of experiments which definitely rule out this type of division hierarchy as one of wide spread occurrence. This leaves us with type (4).

Type (4) offers a compromise between the two extremes of (2) and (3). Those cases in which a typical division hierarchy can be generated from a single isolated cleavage cell lead us to assume that non-differentiating divisions occur at least up to the level in which the cell in question occurs. In other cases ("mosaic eggs"), in which this is not so, we are driven to assume differentiating divisions from the beginning. But in such cases, for reasons given in Part I, we cannot expect differentiating divisions alone, without histological elaboration, to fulfil all the requirements of an interpretation of development in the later slices. Consequently the most reasonable view seems to be that division hierarchies of type (4) are those realized in nature, and this assumption will be adopted in what follows. But it must be remembered that we are referring only to differentiating divisions with respect to *H*-type, all divisions being assumed (for the present) to be non-differentiating with respect to *G*-type. Weismann does not appear to have made a distinction of this kind, and his scheme is not therefore easily comparable with the present one. He assumed that all divisions in the cell-ancestry of the gametes were of type (1), (3) or (4) in Fig. 3, Part II (p. 452), and that all other divisions were of type (2).

We must go back now and review the above considerations in their relation to other developmental processes. In Part I it was pointed out that we could regard development as the outcome of *three* major processes of elaboration, namely:

(1) Cell-elaboration.
(2) Histological elaboration.
(3) The elaboration of *cellular* components.

(1) Cell-elaboration was described in Part II as being achieved through the spatial repetition of an intrinsic cell-pattern. The *cell*-pattern is spatially repeated whenever a cell divides into two *cells* (as contrasted, say, with two *half*-cells), but, on the assumption we have made regarding *G*-type, if a cell c_1 is genetically different from another cell c_2, then, when c_1 divides into c_1' and c_1'', we shall have $(c_1', c_1'')I_\sigma(c_1)$ and $(c_1', c_1'')D_\sigma(c_2)$. There is, therefore, spatial repetition not merely of a "cell-pattern in general," but of a *specific* cell-pattern. When a monkey-zygote divides it does not merely divide into two cells, but into two *monkey*-cells, and so on. But since, with respect to *H*-type, we are assuming that a cell may undergo differentiating division, the cell-pattern will, it seems, be such that it contains elements which are *not* spatially repeated in all divisions, but are capable of being differentially distributed among the two cells into which a given *c*-cell may divide. We might thus be led to suppose that there were two fundamentally different kinds of cell-part, and it may be that only those which are capable of spatial repetition are components (as defined in Part II).

(2) In histological elaboration some-

thing happens in a cell which has not previously happened in cells ancestral to it. This is most evidently the case when there are manifest differences between cells indicating the presence of special cell-constituents. But the expression is here used in a wide sense, leaving open the question whether special cell-components or constituents are involved or not. Relational difference (as defined in Part II) may play a great part in the early stages of development, and all difference arising from histological elaboration *begins* as relational difference.

(3) The elaboration of cellular-components is presumably achieved through a combination of processes (1) and (2). As we have seen, spatial repetition of a specific cell-pattern alone would not yield cellular components. But, combined with differentiating division, provided the planes of division were suitable, it would yield a system of cells analysable into ordered classes of cells according to their different *H*-types. But to carry this through the whole of development would involve (except in the case of the simplest organisms) a prodigious preformation. We are therefore driven to assume that histological elaboration also occurs, so that our first cell-classes resulting from cell-elaboration and differentiating divisions become divided into sub-classes in consequence of histological elaborations occurring in some of the cells and not in others—this difference between them resulting from differences in their relation to other components in the spatial hierarchy. But we require a better way of expressing the relation between a state of affairs in a given *sW* and the state of affairs in earlier slices. Let us first write down a list of what appear to be the fundamental *cell* processes involved, and then we shall see what other notions are required to enable us to deal with the elaboration of *cellular* components. Such a list is especially important when we come to consider the connexion between *G*-type differences and differences in developmental processes as displayed in different division hierarchies.

Cell-Processes

(i) Basic metabolic processes, (including:
(ii) *Maintenance* of specific cell-pattern),
(iii) Spatial *repetition* by division of specific cell-pattern (with or without:
(iv) Differentiating division, with respect to *H*-type),
(v) Histological elaboration,
(vi) Movement.

Now it seems clear that we cannot describe development exclusively in terms of such cell-processes because some of the occurrences we have to describe involve properties which (as was pointed out in Part I, p. 19) are properties not of single cells but of systems of cells. Two systems of cells are of special importance, namely: (1) cell-cones (defined in Part II), and (2) what I shall call cell-series, and by this I simply mean the ordered class of cells constituted by a given cell plus all the cells standing in conv. R_d^p to it, i.e. the cell-ancestors of a given cell together with that cell itself. There is one other notion required for the present, namely that of a *developmental route*, which may be defined as follows: Let $c_1, c_2, c_3, \ldots o$, constitute a cell-series of which c_1 is the representative in a slice sW_n and o is the first member (being the zygote of the division hierachy to which c_1 belongs). Then let R_1, R_2, R_3, ... represent the organic relations in which these members of the cell-series (except o) stand in the successive temporal parts of the dW, and let $E_1, E_2, E_3, \ldots E_o$, represent the environment of these same successive temporal parts or slices of the

division hierarchy. Then by the developmental route of the cell c_1 I mean the whole set of organic relations and environmental relations of the members of the cell-series to which c_1 belongs.

Now consider any cell c in some late slice of a division hierarchy. Let the symbol (*C.G.H.*) represent (i) that it is a cell, i.e. has the cell-pattern, (ii) that it belongs to a certain special class of cells (*G*-type) the members of which are distinguished in some way from all other cells, and (iii) that it also belongs to a certain *H*-type, i.e. to a certain special sub-class within the wider class indicated by its *G*-type. We now have to consider upon what the *G*- and *H*-type of c depend. So far the assumption has been made that the *G*-type of c depends only upon the *G*-type of the zygote o to which it stands in some power of R_d. And since this is equally true of every cell of this division hierarchy, c will not be distinguished from its neighbors in *G*-type. The question of its *H*-type is more complicated. We can conceive the *H*-type of c to be possibly dependent upon (1) its cell-ancestry, and/or (2) its developmental route. That the *H*-type of c depends on its cell-ancestry follows at once from the admission of differentiating division, since it is evident that its *H*-type will depend on the differentiating divisions that have occurred in its cell-ancestry. That the *H*-type of c depends on its developmental route seems to be equally clear from numerous transplantation experiments which consist in allowing cell-series to be generated in a developmental route different from that in which they would have been generated had it not been for the experimental interference.

It will be noted that (*C.G.H.*) cannot represent a possible state of affairs until we indicate how the cell in question is related to other things. Thus (*C.G.H.*)*E* would represent a zygote, a detached gamete or a protozoon (or protist); (*C.G.H.*)R_s(*sW*)*E* would indicate that the cell belonged to a spatial hierachy with no cellular components (although it does not indicate its precise place in the system); (*C.G.H.*)R_s(*P*)R_s(*sW*)*E* would represent a cell belonging to a given cellular component *P* of an organism possessing cellular components, and so on for cellular components of higher orders. These further complications cannot be neglected when the ***relational*** properties of cells (as defined in Part I) are in question.

The following quotations from Weismann (8) will serve to throw into relief the differences between his fundamental assumptions and the modern ones:

> the fate of the cells is determined by forces situated within them, and not by external influences.
> a certain cell in a subsequent embryonic stage does not give rise to a nerve-, a muscle-, or an epithelial-cell because it happens to be so situated as to be influenced by certain other cells in one way or another, but because it contains special determinants for nerve-, muscle-, or epithelial-cells.

For these assumptions I substitute (from the standpoint of the concept of organism) the following:

> the future history of a given cell c which is a component in a spatial hierarchy, depends on (1) its *G*-type, (2) its *H*-type, (3) its organic relations to the rest of the organism, including its relation to the component (if any) of which it is a component, and (4) the external environment.

To which may be added in further elucidation:

> the *G*-type of c depends on the *G*-type of the zygote to which it stands in R_d^p. Its *H*-type depends on (1) the occurrence of differentiating divisions in one or more of the members of its cell-ancestry, and/ or on (2) its developmental route.

Thus the modern view is a good deal more complicated than Weismann's (and this is a good sign), but it is none the less the outcome of a consistent application of the two postulates of causation and of ana-

lytical interpretation to the data yielded by modern experiments. It should be noted that no reference has been made to chromosomes or even to nuclei. To translate "*G*-type difference" into "chromosome difference" is simply to make a further hypothetical application of the postulate of analytical interpretation. But the above reasoning is entirely independent of any such hypothesis, and would still hold if we knew nothing about nuclei. Any one, therefore, who adopts the above two postulates, and the few elementary data and assumptions that have been invoked, is compelled to adopt the rest, provided the reasoning has been correct, whatever view he may take regarding the chromosome hypothesis. This will be clear from a reference to the definitions which have been progressively built up from the beginning of Part II.

It is always much easier to think in terms of cells than in terms of cell-systems, but in dealing with the elaboration of cellular components one is compelled to deal with cell-systems. This is facilitated by the use of the notion of the cell-cone. Considerations of space forbid any detailed discussion here, but a few important points may be illustrated. Suppose we are comparing two slices of the same division hierarchy during the developmental period, call them sW_n and sW_m (sW_n being earlier than sW_m). Then it is evident that there will be cell-cones having their apical cells in sW_n and their basal cells in sW_m, the latter consisting of the classes of cells (in sW_m) which are the contemporary descendants of the apical cells belonging to sW_n. Thus sW_m is analysable into classes of cells consisting of such "bases" of cell-cones. Moreover, the apical cells of such cones in sW_n will also fall into classes which are the basal cells of cones having *their* apical cells in still earlier slices. Now such cell-cones will be either homogeneous or non-homogeneous, a homogeneous cell-cone being defined as one in which no differentiating divisions take place. But such a cell-cone could become non-homogeneous in the sense of containing cells of different *H*-type if some of its members come into such relation with other cones that they undergo histological elaborations which do not occur in the rest of the cone to which they belong. Suppose, for example, that all the epidermis cells of one side of the head in a frog in a certain early slice sW_n belong to the same homogeneous cell-cone, and that all the cells of the outer wall of the optic vesicle of that side all belong to another homogeneous cell-cone. Then a small sub-class of the former class gives rise to a number of cones whose basal cells in a later slice sW_m undergo histological elaboration into lens-cells. And we know that *which*, in some animals, of the cells of the former class of epidermal cells is selected to start these lens-cell-cones depends on which are organically related in a particular way to the optic vesicle cells. Thus a considerable part in development would appear to be played by a combination of (1) the generation of different cell-cones following differentiating division, (2) the diversification of such cones by different histological elaborations in some members of them in accordance with their different organic relations, followed by (3) the elaboration of more cells yielding more homogeneous cones consisting of cells of the newly elaborated *H*-type. An important type of histological elaboration from this point of view is that which expresses itself in increased or decreased rate of cell-elaboration. One cell-cone may differ from another in the time interval which separates the members of the cell-series into which it is analysable. This may be expressed more precisely as follows: Let c_1 and c_1' be the apical cells of the two

cell-cones compared, and let c_4, c_3, c_2, c_1 and c_4', c_3', c_2', c_1' be the two cell-series to which they belong, and which belong to the two cones. Then if the temporal stretch between c_4 and c_1 is shorter than that between c_4' and c_1', and if this difference holds good also for the other cell-series into which the two cones are analysable, then the rate of division in the cone generated from c_1 would be greater than that in the cone generated from c_1'. These, then, are some points in the interpretation of the elaboration of cellular components, but they scarcely touch the surface of the problem. Among contemporary ideas in embryology the Field Theory of Gurwitsch (3) and his school seems to be the only one which shows any promise of dealing at all adequately with this, the most difficult aspect of development.

We can now say something about the second of the questions drawn up at the end of Section II above. This was the the question: How are differences of *G*-type related to taxonomic differences? We have seen that, if $(sW_n{}' dW_1)$ and $(sW_n{}' dW_2)$ are the two slices compared and found to be taxonomically different from one another, and if we can assume that $(EdW_1)I(EdW_2)$, then we shall be dealing with a case of taxonomic difference which is wholly the outcome of a difference of *G*-type between the cells of the two division hierarchies compared. We have also seen that the taxonomic difference between the two slices will be a difference (assuming the postulate of analytical interpretation) of (1) number of cells, and/or (2) relations between the cells (e.g. differences between cellular components), and/or (3) the kinds of differentiating divisions and histological elaborations exhibited by the cells of the two spatial hierarchies. Now since in each case the number of cells, relations between the cells, and the histological elaborations, etc., which the cells exhibit are the outcome of the developmental processes we have already discussed, it follows that the differences between the two spatial hierarchies under consideration will be wholly the outcome of differences in the developmental process in the two cases, and these differences in turn will (assuming the causal postulate) be the consequence of the differences in the *G*-type of the cells of the two hierarchies. Suppose the manifest difference between the two spatial hierarchies is only one of skin color, then the developmental process which is different in the two cases will be that of the histological elaborations in the skin-cells in the two cases. Suppose it is a difference in shape or number of some cellular component, then it will be a question of difference in the complex processes involved in the elaboration of the cellular component concerned, thus involving relations between cells. It is in this case that it is most difficult to understand how *G*-type difference is correlated with taxonomic difference. Suppose we have to do with a difference in size of part or whole, then it will be a question of the rate of division in corresponding cell-cones in the two cases. Thus although, as we have seen, the history of a given cell may depend not only upon its *G*-type, but also on (1) its cell-ancestry, (2) its developmental route, (3) its relations in the spatial hierarchy to which it belongs, and (4) the environment (*E*) of that hierarchy, although this is the case, yet, if we have two division hierarchies with the same environment but different *G*-type, this difference of *G*-type may result in such differences in the early developmental processes as to produce differences in (1) the cell-ancestry, (2) the developmental route, and hence (3) the relations in the spatial hierarchy, of a cell in a much later slice. Consequently differences of *G*-

type may have either a direct or a remote effect on the behavior of a given cell in corresponding slices of two genetically different division hierarchies in the same environment.

We can now discuss the interpretation of a certain type of experiment which has important bearings on the foregoing problem. I refer to those experiments in which an embryonic part is transplanted to another organism of different species and to a site different from that which it normally occupies in the organism to which it belongs, and is then found to develop "*ortsgemäss*" while at the same time retaining its "species specificity." Such experiments have been thought to conflict with current genetical theories. I think it can be shown that this is not the case, (see Part I, p. 18). Suppose we have two genetically different division hierarchies dW_1 and dW_2. Let k_1 be a group of cells in the slice sW_n of dW_1 which (from a knowledge of the properties of the genetic hierachy to which dW_1 belongs) we expect to furnish cell-descendants in a later slice sW_m which will constitute a cellular component P_1 characterised by the particular determinate character d_1 under the determinable δ, Fig. 1A. Let k_1 be a corresponding group of cells in sW_n'dW_2, expected to give origin to a corresponding component P_1, but characterised by a different determinate character d_2 under the determinable δ in

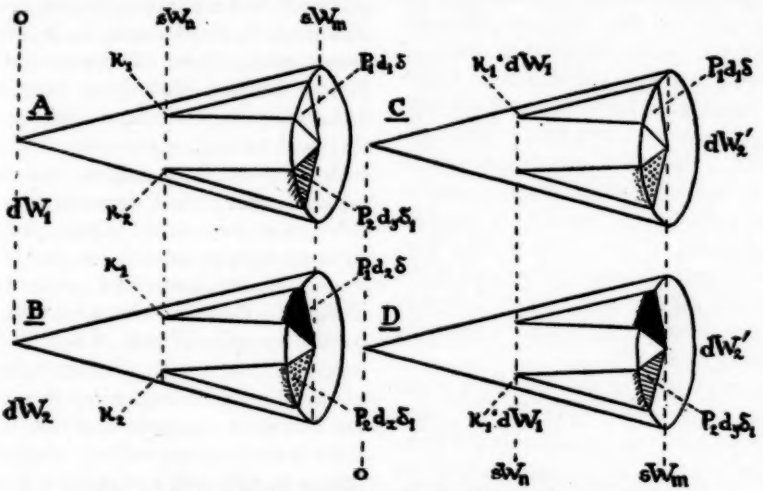

FIG. 1. DIAGRAM OF EXPERIMENTS OF THE TYPE DISCUSSED IN THE TEXT

A four-dimensional division hierarchy is represented by a three-dimensional cone. A plane of the cone perpendicular to its vertical axis represents in two dimensions the three-dimensional spatial organism conceived in abstraction from time (sW). The apex of the cone represents the zygote. The wedge $k_1 - P_1$ represents the system of cell-cones generated from the cell-group k_1 and resulting in the establishment of the cellular component P_1. For further explanation see text.

sW_m'dW_2, Fig. 1B. Let k_2 be another group of cells of sW_n'dW_2 giving origin to a totally different component P_2 characterised by d_2 under determinable δ_1; and let k_2 of sW_n'dW_1 be a corresponding cell-group yielding P_2 (homologous with P_2 of dW_2) but characterised by d_1 under determinable δ_1. Now suppose we find that k_1, when transplanted to the site of k_1 in sW_n of another division hierarchy dW_2' of the same G-type as dW_2, yields P_1 characterised by d_1 under δ, instead of d_2,

Fig. 1C. Then we can say, using the causal formula:

$$D_m(P_1\text{`}\,dW_2', P_1\text{`}\,dW_2)\cdot C\cdot$$
$$D(G\text{-type } k_1\text{`}\,dW_1, G\text{-type } k_1\text{`}\,dW_1)$$

i.e. the difference between the P-components of the two division hierarchies is causally correlated with the difference in the *G*-type of the cells derived from dW_1 and those of dW_2'. The transplanted cells, having the same *G*-type as that of the other cells of the division hierarchy whence they came, have given origin to cell-cones in which the cells have undergone histological elaborations of the same type as they would have done on the original site. This also assumes that the *G*-type is not changed by transplantation, and that the organic relations or developmental routes of the *P*-cells of dW_2' were not sufficiently different from those of the *P*-cells of dW_1 to alter the histological elaborations of the cells derived from the transplanted k_1 cells.

But suppose k_1 of dW_1 had been transplanted to the site of k_2 in sW_n`$dW_2'$, and had furnished cells constituting the component $P_2$ in $sW_m$`dW_2', but characterized by d_y under determinable δ_1, Fig. 1D. Then, comparing, sW_m`$dW_2'$ with $sW_m$`dW_2, we should again have:

$$D_m(P_2\text{`}\,dW_2', P_2\text{`}\,dW_2)\cdot C\cdot$$
$$D(G\text{-type } k_1\text{`}\,dW_1, G\text{-type } k_2\text{`}\,dW_2')$$

The difference in the determinate characters manifested by the two corresponding parts would again be correlated with the difference in the *G*-type of the cells involved. But if we compare dW_2' with dW_1 we have to account for the difference between the behavior of the cell-descendants of k_1 in the two cases, and this can only be correlated with differences between the organic relations of the cell-cones involved in the two cases. If we let K_1 stand for the whole slab of cell-cones involved in the development of P_1 in dW_1 and K_2 for the corresponding slab of cell-cones in dW_2', and if R_1 and R_2 represent the set of organic relations or developmental routes in the two cases (E being supposed to be the same for both) then we have:

$$D(K_1, K_2)\cdot C\cdot D(R_1, R_2)$$

No *G*-type difference is involved, since the cells of K_2 and of K_1 have the same *G*-type. And since P_2 in dW_2' and P_2 in dW_1 are homologous cellular components, present in *both* division hierarchies, there is no difference in them *as* components, e.g. they may both be legs, or skin, or neural tube, or what not. It is only in their determinates under various determinables, e.g. color, that they differ. Consequently *as cellular components which "correspond"* in the two division hierarchies there is no $D(P_2\text{`}dW_1, P_2\text{`}dW_2')$ with which we need to find a correlated pair of *G*-type differences. So long as cells come from division hierarchies which are capable of elaborating a cellular component of the P_2-type there is nothing surprising in their being able to furnish such a component in a different species or genus which is capable of elaborating such a component, so long as the transplanted cells have not already undergone their definitive histological elaboration. For with respect to P_2-elaboration there is no reason to suppose that the two division hierarchies are genetically different, or, in other words, such genetic difference as may exist between them does not affect the processes concerned in the elaboration of a P_2-component as such, i.e. does not affect the early phases of its elaboration, but only the later ones in which histological elaboration occurs in its cells. Thus it is sometimes said that such experiments show that some processes are "species

specific" and others not, and that the latter are not dependent upon the chromosomes. But, in such cases as the above the "other processes" are not processes in which the two division hierarchies concerned differ, consequently it is not surprising that the result of transplantation is such as we observe. The organizer formed by the blastoporic lip, for example, is said not to be "species specific," but is this not simply because all the "species" upon which it can be transplanted have this type of developmental process in common? We can express this by saying that there is no difference of *G*-type involved so far as this process is concerned, whatever may be the truth about the chromosomes. But when we consider the particular determinate character d_s exhibited by P_2 in dW_2' as contrasted with P_2 in the "normal" dW_2, then we do have another correlated pair of different entities, namely the k_1 cells from dW_1 and the different (in *G*-type) k_2 cells in dW_2. Fig. 1 will make these points clear.

We can summarize the chief features of the developmental process in the following general terms: Development is the realization of a pattern in time (like a tune) by the spatial repetition of a specific *cell*-pattern with the consequent elaboration of a *cellular* pattern, constituted by cells in organic relation. This cellular pattern becomes diversified by the elaboration of cellular *sub*-patterns in consequence of the elaboration of *different* cells, either by differentiating divisions, or by histological elaboration in accordance with the different organizing relations set up in consequence of previous differentiating divisions. The pattern of the whole, and of its cellular components, continually changes through the generation of new cell-cones, thus yielding more and more elements of pattern capable of further diversification in the sub-patterns. Development is *dependent* throughout on the environment but, in so far as it occurs in a temporally uniform environment, the *temporal* pattern of a division hierarchy (i.e. its temporal diversification) is not dependent upon environmental *changes*. Development is, therefore, to this extent and in this sense an immanent process.

Before concluding this section a few points of difference between division hierarchies in metazoa and protozoa may be mentioned in passing. The fundamental difference depends, of course, on the fact that, save in respect of its cellularity, a given member of a protozoan division hierarchy is comparable not with a cell but with a temporal slice (sW) of a metazoan division hierarchy. That is to say it does not stand in a relation R_s to any other organic entity. Consequently such a protozoan cell cannot undergo histological elaborations in accordance with its R_s relations since there are none, and there will be no such thing as "*H*-type." As regards *G*-type the same principles hold in both cases. But, although there is no such thing as *H*-type difference, one member of a protozoan division hierarchy may differ from another, even although it does not differ in *G*-type. Such differences will correspond to the contingent relational or specific differences between different division hierarchies in metazoa which result from different environments (environmental taxonomic differences).

IV

The second, fourth and fifth questions raised at the end of Section II are obviously related, and together cover a whole group of problems. They involve all the topics usually embraced under Classification, Phylogeny and Mendelism. Something must first be said about classification and the analysis of classificatory propositions. If our previous reasoning has been correct,

and our basic assumptions suitably chosen, a taxonomic difference between two corresponding slices of two division hierarchies which is wholly the outcome of a genetic difference between the two zygotes, will be at bottom a *cell* difference, i.e. a difference in the *G*-type of the cells of the two organisms. Accordingly a rational classification would be based on this. In actual fact, of course, the traditional classification has not been based on this for the simple reason that we know next to nothing about it. Instead, it has been based on a comparison of the manifest sense-patterns correlated with the two organisms, and this alone tells us nothing about the *G*-type of their cells. In our present method of classification the various taxonomic groups are defined by sets of propositional functions stating the "diagnostic characters." (A propositional function is, roughly, a function which becomes a proposition when some definite entity is substituted for its argument. Thus "x has non-nucleated red blood-corpuscles" is a propositional function which defines the class "mammalia.") This procedure corresponds to a classification of chemical entities in terms only of their manifest characters, without reference to their "properties" (e.g. solubility, vapor density, specific gravity, precipitation reactions, etc.) and is obviously beset with great difficulties. In the first place it makes no explicit reference to the environment, although there must be a tacit assumption that the latter has been "normal." A systematist who was given a "cyclopian" *Fundulus* (and knew nothing about the experiment with magnesium) would either put it aside as "abnormal" or place it in a totally different taxonomic group from that of "normal" specimens, although, in a classification based upon the *G*-type of their cells, two such fishes might be placed in the same group. In the second place, this method usually makes no explicit reference to the temporal extension and diversification of organisms (division hierarchies), and the common restriction to the adult may lead to some strange misunderstandings.

What do we mean by assertions of the type "x is an S," where x is some short temporal slice of a division hierarchy (metazoan), and S is the name of some taxonomic group, some order, genus, species or variety, etc.? We usually mean that x is one of the values satisfying the various propositional functions which constitute the definition of S. But x may not satisfy a single one of these functions, and may yet be called an S. We say that all chordates have a notochord, but no chordate animal has a notochord before it reaches the "gastrula" stage, and no adult mammal has a notochord. Yet we do not for this reason deny the title "chordate" to *Amphioxus* while it is a blastula, nor do we deny it to an adult rabbit. We are thus compelled to introduce the question of temporal diversification, and "x is a chordate" may then (it seems) mean either of two things: (1) It may mean "the visual pattern correlated here and now with x exhibits the features recognized as 'notochord,' " or it may mean (2) "x is a slice of a division hierarchy some *past* slice of which exhibited a notochord." This would meet the case of the adult rabbit, but it would not do for the *Amphioxus* blastula, nor the rabbit before the primitive streak stage. We might then try again and say that "x is a chordate" means (3) "x is a slice of a division hierarchy some slice of which *will have* a notochord." But this is to make an assertion about the future about which we know nothing, not about x. There may never *be* a future slice of the x division hierarchy to have a notochord; x may be trodden on, or dropped into a fixing fluid.

Our classificatory propositions are not hypothetical assertions about the *future* of organisms. We therefore require an interpretation of "*x* is an *S*" which will show it to be an assertion about *x* itself, *whatever slice* of the division hierarchy *x* may be. And clearly, on the basis of our previous analysis, such an interpretation will be provided if we say that "*x* is an *S*" is an assertion about the *G*-type of the cells of *x*. There will thus be two meanings of propositions of the type "*x* is an *S*." They will mean either (1) "*x* here and now satisfies the defining functions of the group *S*," or, (2) "*x* here and now is a cell (i.e. a zygote), or is analysable into cells, having a certain *G*-type, namely, the *G*-type characteristic of the cells of all division hierarchies which satisfy the defining functions of the group *S* in a certain environment." Consequently, when we say "*x* is an *S*" in this second sense we are not saying anything about one particular slice of a division hierarchy, but something which may be said about *any* slice. Moreover, on the assumption that the *G*-type is constant, this assertion makes no reference to the particular environment of the division hierarchy concerned.

The fundamental difference between these two meanings lies in the fact that the first makes an explicit reference to a particular slice (although it may be a "long" slice in the case of the adult) whereas the second, as we have said, is under no such restriction. In consequence of this the first meaning involves the *notion of development*, and the second does not. In terms of this analysis we can now state why it is that we feel that, in regard to a hen's egg in an incubator, *if* it develops at all what hatches out of it will be far more like a fowl than like any other organism, whereas we should be quite prepared to admit that even in the best of incubators it might not develop *at all*. It is because "hatching out like a fowl" depends on the successful accomplishment of the developmental process, which we find by experience to be liable to environmental contingencies. But "being a fowl" in the second of the above meanings of "*x* is an *S*" does not involve the developmental process at all. In *this* sense what is in the egg at the start is as much a fowl as what hatches out of it at the end, and experience has taught us that the *G*-type of cells (to adopt the terminology of this paper) is relatively constant, and remarkably immune from ordinary environmental contingencies. To emphasize this point it will be convenient to call the first of the above meanings of "*x* is an *S*" the *developmental meaning*, and the second meaning the *genetic meaning*, or non-temporal meaning, since it does not (we are assuming) share in the temporal diversification of the division hierarchy.

Lack of space precludes the possibility of giving here detailed examples of the way in which the foregoing analysis enables us to avoid many of the perplexities still current in attempts to bring embryological and genetical data into relation to one another, but a consideration of the following passage from Dürken (2) will illustrate some important points:

> Although there may be no essential difference between racial, specific and even generic characters, yet there is certainly one between the properties by which during development one of the higher organisms becomes a bilateral structure, a fish, reptile, bird or mammal, in comparison with those which cause the color of the hair or of the eyes, the form and length of the body, tail or ears.

It is not easy to know what precisely is intended by an "essential difference" in this passage, but I do not think that such a distinction as is here suggested either need or can be made. Dürken also speaks of "essential" and "inessential" *characters*, and if this simply means that one can get

on quite well with a long or a short nose, but not without a nose at all, then it is easy to understand what is meant. But this is a different matter from saying that there is an essential difference between the developmental process through which one comes to achieve a nose, and that in virtue of which one achieves a long or a short one. I should say that the former was chiefly a question of the elaboration of a cellular component or constituent and was very complicated, whereas the second would chiefly be a question of the rate of division in certain of the cell-cones involved in that elaboration. But as far as dependence upon *G*-type of the cells is concerned there seems to be no need to make any sharp distinction between them, although it is this which Dürken has in mind in the context of the above passage. It occurs to me that one reason for the belief that some such important distinction is necessary is the failure to distinguish the two meanings of "*x* is an *S*," together with some lingering misunderstandings left by the phylogenetic discussions of the last century. If we speak of the process "by which during development one of the higher organisms becomes a fish" we can only be speaking of the *developmental* sense of "*x* is (or becomes) a fish." We are then referring to those developmental processes through which the particular components and constituents are elaborated in virtue of which *x* satisfies the defining functions of "fish." But there is never a time in which *x* is not a fish in the second or genetic sense, and hence no need for anything to "cause" it to become one. When we turn to the developmental processes through which the eyes become green, the hair brown, the body, tail or ears long, we do not appear to have to deal with a situation which in principle is in any fundamental way different from the former. If *x* is a fish in the genetic sense, so it will be a green-eyed fish, or a brown-eyed fish, or a ... etc.-eyed fish in the genetic sense, although the division hierarchy to which it belongs may or may not later realize a slice which is a green-eyed fish in the developmental or first sense—since this depends on environmental contingencies. Some Neo-Lamarckians say that blueness of eyes is an "inherited" character and sunburnt skin an "acquired" one. But "being blue-eyed" and "being sunburnt" either in the developmental or in the genetic sense are not fundamentally different (if the foregoing analysis is adopted). The only difference in this case is that if (at present) a human division hierarchy is to be generated *at all*, up to the slice which exhibits an eye, this can only happen in an environment which, to a high degree of probability, will also ensure that the eye is blue, if the hierarchy is genetically blue. But it may easily go through its whole temporal extent without encountering an environment requisite for it to be developmentally sunburnt, even although it is genetically sunburnt! In order to be developmentally blue-eyed it is necessary to be genetically blue-eyed *and* to encounter a blue-eye-realizing environment. And the same is true *mutatis mutandis* of becoming developmentally sunburnt. That this is not so shocking as it looks will be seen from a reference to the genetic meaning of "*x* is an *S*."

There is, however, one obvious difference between the so-called "essential characters" and the "inessential characters." One cannot have a green eye if one does not have an eye, and one cannot have a long nose if one does not have a nose. But, on the other hand, one cannot have *merely* an eye first of *no* color, or a nose first of *no* length, and then proceed to stick "characters" on to these nondescript parts like tickets on the goods in a shop window. The fact is that parts such as

"eye" or "nose," like the whole organism itself, are four-dimensional temporally extended things, and their temporal boundaries are vague. We speak of the first beginnings of such parts as eye or nose rudiments, leg and tail buds, etc., and these have length and color, and other determinables just as the fully developed parts have. When the color or length, etc., of such a part is spoken of it is the color, etc., of the *fully* developed part that is usually meant. Consequently such characters are manifested relatively late in development, and are regarded as something superficial which *might not* be there, and hence are called inessential. Whereas the *parts*, the later slices of which they characterise, have their first slices early in development and come to be regarded as "essential," because if anything went wrong with them the consequences for later slices of the division hierarchy would be serious. The fact that the developmental process is such that the late slices of a part can only be formed on the basis of the earlier ones does not seem to be any reason for supposing any fundamental difference to exist between the developmental *processes* concerned.

This brings us to the celebrated *Biogenetische Grundgesetz*. (How fond the Victorians were of *Gesetze!* Any generalization, however vague, was dignified by the name of "Law".) In this generalization a parallel is drawn between three sets of data, and this parallelism is based on the formal resemblance of various types of *inclusion relation* which occur in the three sets of data. There is first the set of taxonomic data, with the various taxonomic groups one within another. This is the logical relation of inclusion between classes. Then there are embryological data of the type to which we have alluded, some parts, namely, have a greater temporal extent than others, which are parts *of* them. This is a relation of spatio-temporal inclusion. Finally we have certain phylogenetic relations founded upon palaeontological data. For example, the temporal extent of fishes is greater than that of amphibians, that of amphibians greater than that of reptiles, and so on. This is a relation of inclusion of durations. We can illustrate this a little more fully as follows; (Fig. 2).

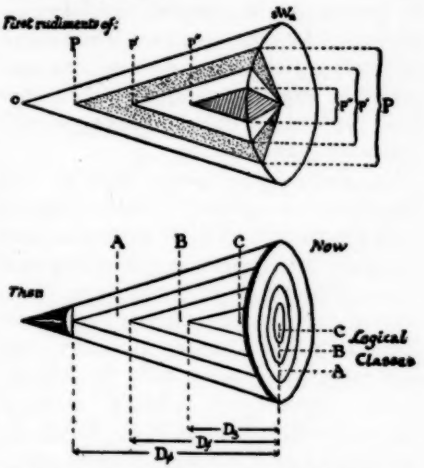

FIG. 2. DIAGRAM ILLUSTRATING THE "BIOGENETIC LAW"

The upper figure represents a division hierarchy in the same manner as in Fig. 1, showing the successive appearance of the sub-components of the cellular component *P*. The lower figure illustrates the time of evolutionary appearance of taxonomic groups of the kind discussed in the text, and the resulting inclusion-relation of the (logical) classes of organisms. For further explanation see text.

(1) *Taxonomic data.* Let ϕx stand for some propositional function which sufficiently defines some animal phylum. Similarly let fx define a class of that phylum, gx an order of that class, Fx a family of that order, and so on until we get to the smallest taxonomic group in which all the members have precisely the same *G*-type (this may be a group with only one member). Now in general we find that the number of organisms satisfying

ϕx is greater than that of those satisfying fx. The number of organisms satisfying fx is usually greater than that of those satisfying gx, and so on. Moreover every organism which satisfies gx also satisfies fx, and every one which satisfies fx also satisfies ϕx. It is because the class (in the logical sense of "class") determined by gx is included in the class determined by fx that the number of organisms satisfying fx is greater than the number satisfying gx. And so on for other groups. It is because the defining character of fx is thus possessed by more organisms than that of gx that the former is called a "more general" character than the latter.

(2) *Embryological data.* Suppose the function ϕx is "x has P," where P is some part possessed by all adult organisms satisfying ϕx, and by no others. And suppose P is one of those parts discussed above which extend over a comparatively large temporal slab of the division hierarchy, their first appearance being early in development. Suppose fx is "x has p'" where p' is a part which is a part of P. Similarly, let p'' be a part of p' possessed by all organisms satisfying gx. (For example, P might be such a part as a paired appendage in vertebrates, p' might be the distal part or hand, and p'' one of the digits.) Then it is clear that P will appear first in development, p' next, and p'' next. The first beginnings of P will be common to the embryos of all organisms satisfying ϕx, p' will be common to the embryos of all organisms satisfying fx, and p'' only common to those satisfying gx. If now we call the group of organisms satisfying ϕx A, that satisfying fx B, and that satisfying gx C, we have the following parallelism between the inclusion relation of these groups, and the spatio-temporal overlapping of the parts and sub-parts:

(C) is included in (B), which is included in (A)

(p'') is preceded by (p'), which is preceded by (P)

It is the combination of these two sets of propositions which underlies von Baer's rules:

1. In development from the egg the general characters appear before the special characters.
2. From the more general characters the less general and finally the special characters are developed.

(3) *Phylogenetic data.* From palaeontological information, coupled with the assumptions of the doctrine of evolution, we might be able to assert the following:

Put D_ϕ for "the duration in which there have existed organisms satisfying ϕx."

Put D_f for "the duration in which there have existed organisms satisfying fx."

Put D_g for "the duration in which there have existed organisms satisfying gx."

Then, parallel with our taxonomic and embryological inclusions we may have:

(D_g) is included in (D_f), which is included in (D_ϕ). Moreover, if, in accordance with the demands of the doctrine of evolution, we consider all the zygotes of organisms satisfying ϕx, from the beginning of D_ϕ to the present, they will constitute a "zygote-cone" analysable into n sub-cones one of which constitutes the zygotes of all organisms satisfying fx from beginning of D_f, and this will be analysable into m sub-sub-cones, one of which is constituted by the zygotes of all organisms satisfying gx, from the beginning of D_g. These relations are represented diagrammatically in the accompanying Fig. 2. It is evident that "recapitulation" results from the fact that p'' is a part of p', and p' a part of P. We cannot have p' until we already have the first slices of P, nor p'' until we already have the first slices of p'. All organisms satisfying ϕx will have the

first beginnings of P, but the fully developed P will not be the same in all organisms satisfying ϕx because only some have p', namely those satisfying fx, and this group does not exhaust the group determined by ϕx, and so on for the smaller groups. But it seems clear that p' is not simply planted on top of an *adult* condition of P, nor p'' on an adult p'. If two organisms belonging to different orders differ in their p''-like parts they will diverge in their developmental processes before the definitive condition of p'' is reached.

Two organisms are said to be related because they both satisfy ϕx. They both satisfy ϕx because their developmental slices up to a certain slice sW_n have a part which is an early slice of P. They both have such a slice because (presumably) the G-type of their cells is *not different with respect to this particular developmental event*. And their G-types resemble one another to this extent because (according to the doctrine of evolution) they belong to genetic hierarchies which, if they could be traced back far enough, would be found to overlap completely in their earlier levels. Thus corresponding to the taxonomic hierarchy we seem to require a hierarchy of G-types.

This, then, is the basis of the doctrine of recapitulation. But it is only exemplified in regard to parts which have the above overlapping nature. It is not exemplified by parts that are analysable into cells which do not have contemporary descendants in later slices, such as foetal membranes. The doctrine does not provide a "causal explanation" of development as an experimental embryologist would understand that expression, but only in the sense in which the doings of Guy Fawkes in the seventeenth century provide a "causal explanation" of the letting off of fire-works by little boys in England on the night of the fifth of November in each year. In so far as it combines the third of the above sets of parallel propositions with the first two it is a historical rather than a causal explanation.

G. R. de Beer (1) has recently pointed out various ways in which (theoretically) a change in the G-type of a genetic succession of zygotes might lead to changes in the developmental processes in the division hierarchies generated from them. In terms of our three fundamental developmental processes we might state some of these as follows: Changes in

A. Cell-elaboration
  (1) In an early slice
    (a) With, and (b) without, disturbance of later slices.
  (2) In a late slice.
B. Histological elaboration
  (1) In an early slice
    (a) With, and (b) without, disturbance of later slices.
  (2) In a late slice.
C. Cellular component elaboration
  (1) In an early slice
    (a) With, and (b) without, disturbance of later slices.
  (2) In a late slice.

It is evident that the earlier the slice affected the more drastic the consequences for later slices, provided the part in question is analysable into cells, which have cell-descendants in later slices. In general one would expect the consequences of such changes to be lethal. That is perhaps why (if they occur at all) their consequences are so rarely observed. In so far as later slices *only* are affected we shall have recapitulation (or repetition in de Beer's terminology) in parts of the type which exhibit such a process (as explained above).

Reverting once more to purely embryological matters we can make a brief review of one of Driesch's arguments in the light of the foregoing discussions. Driesch knows perfectly well that there can be no such thing as an empirical "proof" of vitalism. But he supposes that the terms "vitalism" and "mechanism" stand for two perfectly clear and definite doctrines which (i) mutually exclude one another, and (ii) between them exhaust all possibilities. Consequently he supposes that if one can be *disproved* the other is thereby established. I think it can easily be shown that these assumptions are by no means justified, but let us examine one of Driesch's most powerful arguments, which may be stated (I hope sufficiently accurately) as follows:

(1) No natural entity can increase its degree of multiplicity "of itself."
(2) Harmonious equipotential systems are natural entities in which the degree of multiplicity is increased but not in consequence of environmental occurrences.
(3) Consequently in such systems there must be some "agency at work" which is "responsible" for this increase in multiplicity.
(4) There are some embryos in nature which are harmonious equipotential systems.
(5) Therefore in some embryos there are "agencies at work" through which their multiplicity is increased.

If any of these premises can be denied the argument collapses. I have already pointed out the difficulties of establishing proposition (4) empirically, because our current a priori postulates would lead us to deny that embryos, as wholes, can be equipotential systems, but let us leave this and consider the first proposition. Driesch regards this as an unshakable dogma. But the following considerations suggest that, if the developing organism is conceived under the "concept of organism," it is not so inevitable as it seems to be at first sight. We have seen that an organized entity having components standing in internal organizing relations will have its degree of multiplicity increased if (i) the *number* of its components is increased, (ii) if the complexity of the *relations* in which those components stand to one another is increased, and (iii) if the *intrinsic patterns* of the components become different from one another (yielding intrinsically different components in the whole). Now in the generation of a division hierarchy in Metazoa we appear to have: (a) increase in the number of components by spatial repetition of the specific cell-pattern of the first member (the zygote); (b) increase in complexity of relations as more members appear; and (c) the becoming different of the components either by differentiating divisions or by histological elaboration, and, in consequence of this, the elaboration of components of a higher order (cellular components). Thus granted (i) the possibility of spatial repetition of a pattern, and (ii) the possibility of differentiating division and histological elaboration, and (iii) that, apart from these primary processes, there is no need to appeal to "agents" of *any* kind, and Driesch's argument is undermined, and the old puzzle of "preformation" and epigenesis" largely evaporates. If we do not need to invoke "entelechies" for the interpretation of cell-division this alone will suffice in principle, because even this process will yield an increase in multiplicity in succeeding slices of the division hierarchy. Neither the "preformationists" nor the "epigeneticists" knew anything of spatial repetition of pattern or of histological elaboration, since such processes occur only in living cells, and there was

therefore nothing in the experience of the early embryologists to suggest that such things occurred. These considerations illustrate the dangers of framing biological theories on ideas borrowed from outside the data of biology. From the point of view of the analysis of developmental processes given above there is no need to regard the zygote as a mystery-bag of "potencies" which, at their appointed hour, proceed to blossom out into the "characters" which they are said to be "for." We require to conceive any given cell as exhibiting one or more of the six fundamental cell-processes described in Section III above, in accordance with (1) its *G*-type (2) its *H*-type, (3) its relations in the spatial hierarchy, and (4) the environment of the latter. *All* these are among the "factors" of development, and all are liable to environmental contingencies in varying degrees, (1) being the most constant, and the only one which persists unchanged (usually) throughout development.

I do not wish to give the impression that the developmental process is simple; we are obviously only at the beginning of understanding it. I have already emphasized the special difficulties of dealing with the elaboration of cellular components. But I do suggest that the above analysis helps to remove some of the *obstacles* to an understanding of development which have been created by notions bequeathed to us by the older embryologists.

V

Little space remains for a discussion of the logic of Mendelism. Its chief importance lies in its bearing on the question of how we are to conceive "*G*-type" of cells. The fact that, from the union of different pairs of gametes from the *same* two division hierarchies (which are not genetically different) two or more *genetically different* kinds of zygotes may result, *compels* us (when coupled with the causal postulate) to admit the occurrence of *differentiating division with respect to G-type* in some cell or cells which are ancestral to such gametes, and this contradicts our fundamental assumption that the divisions in a division hierarchy are non-differentiating with respect to *G*-type. This difficulty is removed when the division is assumed to occur (as it is on cytological grounds) in a *c*-cell which stands in conv.R_d^2 to a given tetrad of gametes. This assumption leaves the foregoing arguments, as far as development is concerned, entirely unaffected.

It is possible to state the Mendelian results in cases where different characters under *one* determinable are involved, in terms of different or non-different gametes, without saying anything "absolute" about them. But when we consider cases involving two or more determinables the situation becomes more complicated because we have to assume that a given gamete may be either singly or doubly different (in the case of two determinables) from another, and it is difficult then to interpret the resulting ratios without invoking the postulate of analytical interpretation, and thus assuming that the gametes are analysable into cell-components which are different or non-different as the case may require. We are thus committed to a "particulate view" and this has been objected to by some biologists. But such objections appear to rest on a failure to distinguish between a particulate theory from the standpoint of genetics and a particulate theory from the standpoint of embryology. Such objections are valid enough against the particulate theory of Weismann, but this theory did not itself discriminate between the requirements of genetics and those of embryology, whereas the modern particulate theory is a purely genetical theory. This is clearly recog-

nized by T. H. Morgan (7). The "particulate view" presents no difficulties to the "concept of organism." We have already seen that this concept *requires* that the organism should be analysable into a hierarchy of components, with the emphasis as much on the hierarchy as on the components. If the organizing relations are given equal weight with the components it is difficult to see what objections can be raised to so-called "particulate" theories. For, in any case, if once *cells* as components are admitted you have already admitted that the organism is in some sense "particulate," and it then seems arbitrary to draw the line below the cell-level.

I have postponed any reference to Mendelism and its consequences to the end because I wished to show how very largely the main features of our current genetical theory are really independent of it, and owe their present form to considerations of a much more general and elementary nature. I have tried to analyse out the principal postulates, assumptions, and empirical data upon which they rest. From this analysis we can see that anyone who wishes to oppose the predominant theory in genetics begins at the wrong end if he begins by attacking the chromosome theory, because the predominant theory is logically independent of the chromosome hypothesis, and its main features are even logically independent of Mendelism, as will now be clear. In consequence of this an opponent who begins at the wrong end may very easily find himself denying something which is a necessary deduction from premises which, in any other context, he would freely admit. I hope my analysis may go some way towards avoiding unnecessary disputes which rest on a failure to understand this point. The proper place for a critic to direct his attacks is at the beginning, namely, at the causal postulate and the postulate of analytical interpretation, and this is a very difficult thing to do, although even a priori postulates should not be regarded as eternally fixed and unalterable. Our attitude towards them, at least, has changed in the course of history, even in the brief span during which human beings have indulged in thinking.

The controversy from which we began in Part II—the question of the "importance" to be "ascribed" to "environment" and "heredity"—should now appear in a clearer light. Is there any sense in asking how many centimeters of a man's nose are "due to heredity" and how many are "due to environment?" Is it not like asking how much of the volume of a gas is "due to" pressure and how much "due to" temperature? This can hardly be what people really have in mind when they raise such questions. Only with regard to two corresponding slices of two division hierarchies could we say, assuming that they had the same environment throughout, if they were manifestly different, that they were taxonomically different, and that this was wholly the consequence of a genetic difference between their zygotes. Or, if we assume that the zygotes were not genetically different we could say that the taxonomic difference was wholly the outcome of environmental difference. But clearly, the developmental processes in each, through which the taxonomic difference of the two slices compared is realized, are in both cases *dependent throughout* on the contemporary environment. We can contrast the two views which H. S. Jennings (4) discusses in the little book referred to in Part II. Jennings describes the view he is opposed to as follows:

> Every individual, therefore, came into the world with his characters fixed and determined. His whole outfit of characteristics was provided for him at the start; what he should be was pre-ordained; predestination, in the present world, was an actual fact. Environ-

ment might prevent or permit the hereditary characters to develop; it could do nothing more.

And the view to which Jennings subscribes is stated as follows:

It is not true that what an organism shall become is determined, fore-ordained, when he gets his supply of chemicals or genes in the germ cells, as the popular writers on eugenics would have us believe. The same set of genes may produce many different results, depending on the conditions under which it operates. True it is that there are limits to this; that from one set of genes under a given environment may come a result that no environment can produce from another set. But this is a matter of limitation, not of fixed and final determination; it leaves open many alternative paths.

Now it is perfectly plain from these two passages that the whole dispute rests on a failure to discriminate between the two meanings of propositions of the type of "*x* is an *S*" as discussed in Section IV above. The first passage is obviously concerned with "*x* is an *S*" only in the *genetic* sense. Only in this sense can we say that what *x* "is" is "fixed and determined" at the start. If taken quite literally the first passage would lead to ridiculous consequences. It would mean that a man could have either a particular length of nose, according to the *G*-type of his cells, or, in cases where the environment "prevented" this, he would have *no nose at all!* And if this were the case we should expect to see many more people going about with holes in their heads or flat patches of skin in the place of a nose than we actually do. This view is so obviously silly that the people who hold it (if they are correctly reported) can hardly mean what they say. There is, however, one thing to be said in justice to the "popular writers on eugenics," namely, that if the view attributed to them above is correct, then the *things they say* about eugenics will *also* be "fixed and determined from the start" and cannot be attributed to defective education, but simply to the peculiarities of their own "supply of chemicals."

Thus we can only say that an organism's characters are "fixed and determined" if we can say that the environmental contingencies which it will encounter as each successive slice is realized are fixed and determined. But even if this were the case we could not tell what the characters of future slices would be from a knowledge, however perfect, of the properties of the zygote alone. We should also have to know the laws of the environmental contingencies which each slice is to encounter, and also the laws governing the organism's response to those contingencies according to the slice of the hierarchy which happens to be realized when it encounters them. And of course we cannot speak of the organism as "having its whole outfit of characters provided for him at the start." The zygote has only the characters appropriate to it as such, and each later slice has the characters appropriate to *it* in accordance with its environment. There are no such things as "hereditary" characters. All these modes of expression date back to a pre-scientific stage in the history of genetics, and these fruitless discussions will continue until they are abandoned (cf. Part II, p. 442).

What people really have in mind when they discuss the relative "importance" of "heredity and environment" is usually the question of whether the *G*-type of cells is liable to change with a changing environment. Most of our experience leads us to think of it as being remarkably constant, and we begin by conceiving it in this way. But if we adopt the doctrine of evolution we are compelled to modify this belief. Moreover, there are the recent experiments which seem to require the assumption of such changes. The whole question is extremely obscure and

our ignorance in this region is profound. The problem which most exercises Neo-Lamarckians is the question: How is "adaptation" involving change of G-type in correlation with changing conditions of life or habit possible? Neo-Lamarckians are people who believe that it is not only possible, but that there is experimental evidence that it actually happens. Others deny that the experiments require such an interpretation, and some deny that it is possible. But such a question cannot be discussed without first undertaking a thorough analysis of all the difficult problems raised by the notion of adaptation and the consequences of the doctrine of evolution. We might find, after such an investigation, carried out independently of the analysis of genetical and embryological ideas, that we were led to make different assumptions from those suggested by the latter, and this might indicate ways in which adjustments could be made to enable the requirements of both lines of approach to be satisfied. But until this has been done in a much more thorough manner than has yet been contemplated it seems premature to hope for such a comprehensive synthesis.

It will have been noted that I have tried to state all the important propositions in terms of the relations of difference and non-difference. This keeps close to the actual procedure of thought in an experimental investigation, and enables us to avoid having to make "absolute" or "metaphysical" assertions about the entities involved. It has been said that "genes only exist in the minds of geneticists." But if this were so genetics would be a branch of psychology, and this does not seem to be the case. If it *is* so then genes are clearly not the things that geneticists are talking about. A "gene" is simply ***that which is different*** in two cells which "belong to different G-types" or "are genetically different." If you go on to add that ***that which is different*** in such cases in an "entelechy," or an "unconscious memory" or a "supply of chemicals," then such further assertions are significant only if they lead to the discovery and systematization of further knowledge relating to data of the same sort as those which led to the assumption of the genetic difference in the first place, i.e. something perceived. They do not take us any nearer to a hypothetical realm "behind the scenes" where the "secrets of nature" are revealed.

My chief aim in these articles, apart from the attempt to clear up *particular* muddles in relation to these topics, has been to illustrate and urge the *general* desirability of devoting some little attention to the linguistic and logical aspects of biology. If we are willing to take endless trouble, and exercise unlimited patience, in order to attain precision in experiment, it seems strange that we should grudge the effort needed for a precise analysis of our logical procedure, and for the accurate expression of our results. What seems to be a common point of view has recently been expressed as follows:

> The nature of the research worker is such that once a scientific puzzle clears itself up he loses real interest in it. He grudges even the time for writing up his results in full and is impatient only to get to grips with the further problem.

Within its limitations this is doubtless a highly meritorious attitude, but it clearly needs supplementation (if the scientific world is not to be peopled entirely by hermits and solipsists) by those who do not grudge the time requisite for the clear intercommunication and interpretation of results. If there is too little time to write up results "in full," there is surely no time at all for the writing of misleading

popular books, or for quarrels which are largely the outcome of the neglect of just those precautions which are an essential pre-requisite of any exact theoretical biology.

Our language, if it is to be adequate to deal with a given realm of fact, must have a logical structure of the same degree of multiplicity as that of the realm of fact itself. It is therefore useless to complain in one breath that biology is backward because organisms are so complicated, and then to refuse in the next to entertain complex ideas, or to construct a language which is capable of giving expression to them. A discussion of embryological topics and their relation to genetics with our current conceptual and linguistic apparatus is like performing a modern surgical operation with a pair of nail scissors and a potato peeler.

I hope the foregoing analysis will make some amends for the very muddled discussion of the same topics in the first part of Chapter IX in my book *Biological Principles*, which was muddled because I had not then freed myself sufficiently from the difficulties inherent in our current notions and language.

It is fatally easy to make mistakes in these matters. If, therefore, any reader is able to detect flaws in the above reasoning I shall be very grateful to have them pointed out.

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THE PRESENT STATUS OF THE PROBLEMS OF ORIENTATION AND HOMING BY BIRDS

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THE phenomena of migration and homing are quite distinct and much confusion has arisen from the fact that they have frequently been treated together in theoretical discussions. It should be noted that in the first migration of a bird's life it flies from the region of its birth to a region to which it has never been before. This alone distinguishes it from homing. The latter phenomenon, though it may involve flight *from* a point never before visited, always involves flight *to* a known point. The goal is a nest or côte where the animal has succeeded in establishing itself. There are other differences. Migration is a distinctly seasonal affair. Homing, in certain species, can be demonstrated at any time of the year. In fact it may be conjectured that *all* species will attempt to home at any time of the year during which they possess an established home to which to fly. Species vary widely, however, with respect to the distance over which they can successfully home.

There are two questions which must be answered regarding both migration and homing. These are: (1) What is it that impels the bird to home, or to migrate? (2) How does the bird succeed in finding its way in the homing or migratory flight? Unfortunately, only the first has received emphasis in discussions of migration, and only the second in discussions of homing. With respect to the first question, it may be conjectured that migration and homing have little in common. With respect to the second they probably are related, for it is extremely likely that whatever means of orientation a bird possesses are utilized in both homing and migratory flight.

We shall not discuss the question of what impels birds to home. Investigators seem simply to have accepted the fact that they do home and have not volunteered explanations. But the problem of how a bird finds its way home has given rise to a number of hypotheses which must now be considered. Although homing over at least short distances could, no doubt, be demonstrated in any bird possessing at the time an established nest, it is the pigeon which has been rewarded with the most attention since this semi-domesticated bird has evidenced an unusual capacity. The facts, briefly, are these. Certain pigeons, but especially the older and previously trained individuals, succeed in returning to their côte when transported to and released from a distant point even though they may not have previously visited the release point. The usual classification of the hypotheses advanced to cover this behavior is based upon the stimulus which is assumed to orient the bird. Our discussion will make use of the following headings:

I. The assumption that stimulation emanating from the côte itself directly affects the bird.

II. The assumption that the bird is sensitive to slight magnetic variation.

III. The assumption that retention of the

effects of stimulation received during the outward journey results either in exact retracing of that journey or in an immediate orientation toward the côte.

IV. The assumption that, upon release, the bird flies without orientation until it encounters stimuli (usually visual) which it had encountered on previous flights, and that these stimuli release those responses which had formerly been successful.

HYPOTHESIS I

Hachet-Souplet (12) is one of those who venture the suggestion that birds orient as a result of direct stimulation by the goal. He calls attention to the extremely acute eyesight of birds and suggests that this stimulation is visual. He supposes that a bird flies to great heights in order to get this direct view, but that such heights need not be as great as would be supposed because of the refraction of light. The view from tremendous distances, even though dim and vague, will arouse in the bird the feeling, "*déja vu.*" He cites instances of homing birds which, upon being released, have flown upwards in spirals until they disappeared from view. This hypothesis is perhaps adequate for all shorter flights. No doubt a bird will respond to direct visual stimulation by the goal when such is available. That homing over distances of several hundred miles can be accounted for on this basis alone is quite unlikely. On data supplied by Johnson on the visual acuity of the chick (14) Watson and Lashley (38) have calculated the size of an object which could be distinguished at various distances. At a distance of 582 miles, for example, there would be comprised in a visual angle of 4′ (the limit of acuity in the chick) a distance of 6.8 miles. There is no reason to suppose, however, that the chick's visual acuity can fairly be taken to represent that of birds capable of extended flight. Watson and Lashley present further calculations indicating the altitude to which a bird would need to fly in order to be directly stimulated by light reflected from the goal. At distances of a hundred miles or more the necessary altitude is beyond that to which birds customarily fly. Duchâtel (in a letter quoted by Hachet-Souplet (9)) avoids the latter difficulty by assuming that birds are responsive to infra-red rays and by further assuming that these rays follow the curvature of the earth. Unfortunately, neither assumption is true (36).

The recently proposed theory of Casamajor (1) should be mentioned under this heading since it may involve the supposition that from the region of the côte there may emanate characteristic magnetic influences which the bird can distinguish from other such influences. Casamajor's suggestion will be considered in the next section.

HYPOTHESIS II

The chief reason for advancing the assumption that birds are sensitive to electro-magnetic stimulation is to be found in the difficulty of explaining their behavior in terms of their known senses. Thauziès (29) and others cite cases wherein delay in the return of homing pigeons coincided with the occurrence of electrical storms. Such data as exist are entirely inadequate. Besides, such storms might affect the bird through other modalities, since they are accompanied by changes in atmospheric pressure, general visibility, and so on. Maurain (17) has called attention to the fact that organisms, generally, are insensitive to far more intense stimuli of this sort than those which are assumed to guide the bird.

Were birds dependent upon such stimuli in their orientation they would become most confused when flying in the neighborhood of electric cables. There has been no indication of such confusion. In a recent article, however, Michel (19) describes behavior in pigeons which is interpreted as demonstrating sensitivity to electro-magnetic influences. When the pigeons in the course of a homeward flight approached a radio station which was at the time sending messages they became confused, ceased their direct homeward flight and flew about at random. Only when they happened to fly beyond the immediate neighborhood of the station did they resume the homeward flight. Such bewilderment was not observed when the radio station was not operating. These observations suggest that a systematic study of the matter should be made.

An argument frequently used against the contention that birds are sensitive to magnetic disturbances is that there is no known organ which might be considered the receptor. The semi-circular canals have been suggested but experimental evidence that they are affected by such disturbances is wanting.

Even supposing birds to be capable of responding to slight variations in the earth's magnetism, it is difficult to understand how they could make use of this capacity when released at an unknown point. A compass would be of no use without a chart. Viguier (33) has assumed that a bird possesses such acuity that it is capable of detecting the angle of "dip," i.e., the deflection from the horizontal of the magnetic force. Since this varies only from 90 degrees at the magnetic pole to 0 degrees at the magnetic equator, it should be noted that a bird would have to discriminate differences in deflection of well under one degree in order to estimate its latitude within a hundred miles. It is not clear how its longitudinal position could be ascertained. Casamajor (1) claims a far less specific and accurate sensitivity to terrestrial magnetism. He suggests that when a bird lives for a time in one place it becomes accustomed to certain sensory cues characteristic of that place alone. When released at a distance from home it flies in one direction and then another but constantly selects the direction which results in cues most resembling those of the home region. While recognizing that there are many factors operating, he believes terrestrial magnetism to be the most important. In support of this belief he presents the results of his own observations and those of others which indicate the unimportance of cues of other modalities. Finally he reports instances in which pigeons equipped with magnetized headgear have failed to home while similar birds equipped with non-magnetized headgear have been successful. Verdict on Casamajor's suggestion must await further data.

HYPOTHESIS III

Reynaud (23) (24) (25) has suggested the retracement theory. The bird is supposed to register in some way all the directions followed on the outward journey, even when it is passively transported. These impressions (chiefly of semi-circular origin) are in some way stored up and when the bird is freed they are released in the reverse direction, guiding it home over the same course. In favor of this theory are presented instances of birds brought to the release point by a circuitous route which required more time to home than those brought to the same point directly. It has not been difficult for other writers to collect as much similar evidence against the theory. Birds which have died on the homeward journey have occasionally been found along the route

over which they had been taken on the outward journey. On the other hand, such birds have been found at many other points including those directly opposite the direction of the côte. Bonnier (as quoted by Watson and Lashley (38)), being persuaded that sometimes pigeons homed so promptly that they could not have retraced, adopts a supplementary theory to account for such instances. He assumes that throughout all the twistings and turnings of the outward journey the bird holds fast to the memory of the *direction* of its côte. If it has succeeded in making exact allowance for all such change in direction, it will, at the moment of release, be able to fly at once in the correct direction.

These theories are susceptible to experimental test for, if the bird is dependent upon sensory cues received during the outward journey, elimination of these cues should render homing impossible. Exner (8) reports successful returns by birds treated during the outward journey in the following ways: anaesthesia; rotation; electrical stimulation.

HYPOTHESIS IV

Schneider (28) claims that homing pigeons do not possess an inborn "sense of direction" or any mysterious sensory capacity, but that they gradually acquire a knowledge of the topography of, first the immediate environment of their côte, and then a greater and greater portion of the surrounding country. He believes that this topographical memory applies principally to prominent visual cues such as large optical patterns formed by hills and valleys and villages. A bird released in unknown territory is at a loss until it happens upon some familiar visual pattern whereupon it orients itself accordingly and flies homeward.

The evidence for this theory is extensive but can be summarized under three headings.

1. *Homing ability is vastly improved by training.* Pigeon fanciers find that only after a long training period are their birds able to home successfully over long distances. Young birds or birds raised in confinement are poor homers. Such training must strengthen the bird's tendency to seek its home and must increase its flying ability. But it probably also familiarizes the bird with the territory over which it is trained. That this is an important factor in determining further homing is indicated by the fact that training in a given direction does not result in as successful flights from points in the opposite direction. Riviere (26) emphasizes the painstaking care with which racing pigeons are trained. They are treated at first "like fools" but the farther the training takes them and the more experience they have, the bigger jumps of unknown country they may safely be tested upon. He also stresses the fact that racing pigeons are trained and raced entirely in one direction from the point of release to the home côte.

2. *There is great variability in the time of returns.* When a large number of pigeons from the same côte are released a few minutes apart it would be expected that they would return at approximately the same time were they all being guided by a cue operative at the moment of release (as assumed in other theories). Any variation would be due to individual differences in flying speed. As a matter of fact the first bird home may arrive twelve hours after the release while the others may come straggling in one, two, three days, or a week later. This great variation in the time of return is what would be expected if it is true that the

birds fly about (singly, since released singly) until they happen onto familiar cues.

3. Increase in the length of distance to be covered increases the average elapsed time out of all proportion. Were homing determined by cues effective from the moment of release we would expect that doubling the distance to be flown would little more than double the time required. It more nearly quadruples the time required, on the average. Again, this is what would be expected according to this theory, since the amount of territory to be covered by a bird before it might chance upon a familiar object would be increased in more nearly a geometric than an arithmetic ratio with increase in distance from the côte. Hodge (13), especially, has emphasized the point that the time taken for the return is usually far in excess of that which would be required by the bird to fly in a straight line, even at a leisurely pace, from the point of release to the côte. He furnishes estimates indicating that the elapsed time is usually more than sufficient to permit the bird to fly in ever widening spirals until it must of necessity fly over a point familiar to it. Were birds capable of direct flight home over unfamiliar territory it would be expected that they would utilize this ability to reach food and protection by more frequent rapid returns.

Of the experiments in support of the hypothesis that the released bird wanders pretty much at random until it happens upon known landmarks, there is one which deserves specific mention. Rabaud (21) reports that a group of pigeon fanciers released at sea 5000 pigeons at distances of 146,200,300 and 500 kilometers west of Croisic. The results show that the speed of the return flight decreases with an increase in distance far more than could be accounted for by fatigue. For the 200 kilometer group it was 75–99 kilometers per hour, for the 300 group it was 60 and for the 500 group, 40. Likewise the percentage of returns diminished sharply with increase in distance. Of the 1500 released at 500 kilometers but 300 returned in two days. The remainder were found scattered in all directions (England, Spain, Portugal, Algeria, Cape Verde, Egypt, The Caucasus). "In short," concludes Rabaud, "return to the point of departure became a matter of chance."

Lest the impression be given that the theory under consideration is in all cases adequate, the work of Watson and Lashley (38) on homing of untrained birds will be reported at this point. These experiments are among the very few on homing that are above severe criticism. The subjects, noddy and sooty terns, were captured during their brief breeding season on Tortugas Island (on the Gulf of Mexico, 65 miles west of Key West). These were marked and transported by boat to the points of release by one observer while the other kept watch of the birds' nests and noted the returns. If the returns were made in a straight line they would in every case be over water the whole distance. From most of the release points the birds might have flown within sight of land practically the entire distance, although this course would have been somewhat longer. In no case was the flying time so short as to rule out this possibility. In general it is to be noted that with increase in distance there is decrease in the percentage of returns. The data on speed of return are not entirely reliable as the writers point out that several hours or days might sometimes have passed between the return of a bird and its observation. In the following summary of these flights the data on the two species of terns have been combined since there were not significant differences.

Certain flights (New York, Mobile) have been omitted since the lack of returns were apparently due to the poor condition of the birds upon release and to shortage of food supply. The longest flight (855 miles) was from Galveston, Texas.

Distance in statute miles	No. birds released	No. returns	Time
19½	2	2	2½ hours
44½	2	2	1¾ hours
65½	26	24	13 h. to 11 d.
108	4	4	1 d. to 1½ d.
418	2	0	
461	7	2	3 d.
585	10	8	3 d. 22½ h. to 7 d. 21 h.
720	2	2	11 d. 13 h. & 17 d. 2 h.
855	10	3	6 d. 2 h. to 11 d. 23½ h.

Although it is true that these birds were "untrained," there is no evidence that they were entirely unacquainted with the regions of the flights. It might be argued that those few birds which succeeded in homing over the longer distances had flown without specific orientation until they had chanced upon a stretch of the shoreline with which they were acquainted. The elapsed time in every case was ample for a vast amount of random exploration. The data presented are insufficient to disprove this possibility. Nevertheless, they suggest the operation of some means of orientation at present unknown.

While the learning theory (Hypothesis IV) might refer to stimulation from any modality, it is the visual which has been particularly emphasized. Many writers have noted that homing is most prompt on days of good visibility, that fog and night delay returns. Blind birds appear incapable of homing even over short distances. After a snow-fall it has been reported that returns are considerably delayed, presumably on the basis of the changed appearance of the country side and the disguising of landmarks. Observational and experimental investigation has indicated that, as compared with other animals, the sense most highly developed in birds is vision. It would seem reasonable to suppose, therefore, that it would be an important factor in homing. On the other hand it is obvious that this would not exclude the use of other available cues.

In addition to the four main conjectures discussed above, it is also possible that there exist certain air conditions sufficiently constant to serve as cues by which flight may be directed. Cathelin (3) suggests that further study of the equinoctial air currents, which he calls "The Gulf Stream of the aerial sea," may reveal conditions which would serve as adequate guides. However, this theory seems to be more applicable to the problem of migration than to that of homing.

Cyon's hypothetical Spürsinn (5), an assumed acute sensitivity to temperature and wind direction having its seat in the nasal region, seems to be definitely disproven by experiments of Watson and Lashley (38) and need not occupy us further.

In summarizing it should be emphasized that no one of the four hypotheses advanced or even a combination of these can be accepted as an adequate description of all homing behavior. Direct stimulation by the côte itself (Hypothesis I) and the maintenance of an orientation toward the côte during the outward journey (Hypothesis III) may apply to very short homing flights but can hardly apply to those over long distances. There is as yet far more evidence against than for sensitivity of birds to terrestrial magnetism (Hypothesis II). The learning theory (Hypothesis IV) appears to be more generally applicable to homing behavior than do any others.

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MITOGENETIC RAYS

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THE problem of light emission in vital processes and in chemical reactions has often been discussed, but because of contradictory experimental results and a tendency toward an over-speculative interpretation of them much of the published work on the subject has been received with skepticism and for the most part soon forgotten.

In recent years, however, new interest has been aroused in the problem by the discovery of biological radiation and its influence on cell division. The pioneer in this work was Alexander Gurwitsch of the Histological Institute of the First Soviet University in Moscow. Since the first publication in 1923 more than forty papers on this subject have come from the pen of Gurwitsch and his co-workers. Of these, two deserve special mention. One, entitled "Das Problem der Zellteilung physiologisch betrachtet" (1) summarizes the work in this field up to 1926. A later survey article in *Protoplasma* (2) reviews the subject up to the year 1928. Inasmuch as no adequate summary of the researches carried on in this interesting field has been published in English it has seemed to the present authors worth while and desirable to prepare such a review.

Gurwitsch defines the mitogenetic effect as the action at a distance of one object upon another living object, producing an increase in the intensity of cell division in the second object. The former agent is designated the *Sender*, the latter is called the *Detector*.

The number of investigators working on this problem is steadily increasing. The experiments of Gurwitsch were first repeated by J. and M. Magrou, later by Reiter and Gabor, and also by Siebert and others. These investigators have verified the findings of Gurwitsch, in essence at least. Others, however, Rossman for instance, obtained negative results. These experiments will be discussed in some detail.

GURWITSCH'S EXPERIMENTAL TECHNIQUE

The original experiment of Gurwitsch will serve to illustrate what is meant by the mitogenetic effect. Bulbs of the common onion (*Allium lepum*) were allowed to stand in water for some time. After the roots had grown to a length of twelve to thirteen centimeters they were examined and the one that was most perfectly formed and had the most symmetrical tip was chosen for experimentation, and the rest cut away. A close fitting glass tube was then slipped over the single remaining root, which was supported in a horizontal position and kept moist during the experiment. This root served as the sender. A second root, to serve as detector, was prepared in the same way and placed in a glass tube; this time, however, the root extended beyond the length of the glass tube for a distance of about five millimeters. Over the protruding end of this root another glass tube was placed, leaving, however, about two millimeters of the root exposed. This preparation was, of course, also kept moist.

The two roots were next brought close

together, the detector in a vertical position, the sender in the horizontal position with the axis of the sender pointing directly at the exposed section of the detector root. This adjustment was very carefully made by means of a telescope so that "the projected axis of the sender root struck the detector exactly in the median line." The whole set up was held firmly in position by a rigidly built frame. It was most important that the adjustments should be carried out with extreme care since on them the success of the experiment rested. After remaining in this position from two to three hours the sender root was removed, the detector allowed to stand a certain length of time, then the direction of induction was marked and the root was prepared for sectioning. The last five millimeters of the root were cut in longitudinal sections, parallel with the direction of induction, and the number of mitotic figures to the right and left of the median line counted. The experiment revealed "a regular sharply circumscribed preponderance of mitoses in the center of the induced side of the root." The excess of mitotic cells on the side toward the sender over those on the opposite side was about fifty per cent. The results of a determination are given in Table I.

TABLE I

Induced side	29	36	58	55	62	56	50	62	36	33	39	37
Not induced side	33	31	40	36	35	42	42	36	40	35	39	41
Difference	−4	5	18	19	27	14	8	26	−4	−2	0	−4

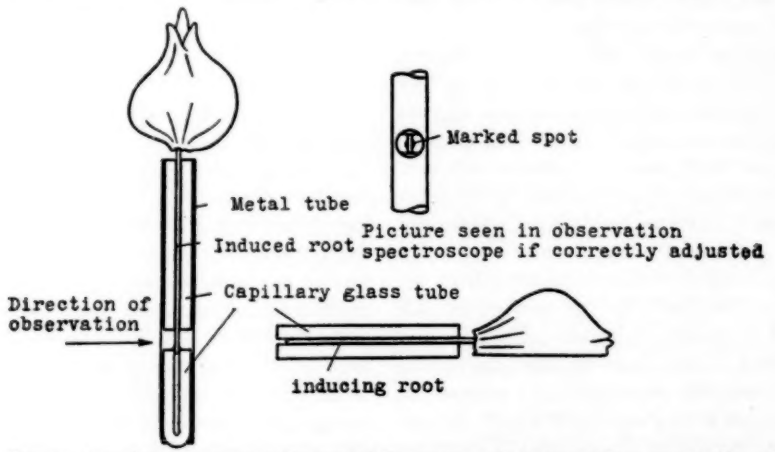

FIG. 1. EXPERIMENTAL ARRANGEMENT FOR GURWITSCH'S FUNDAMENTAL EXPERIMENT (INDUCTION OF ONION ROOT BY ONION ROOT THROUGH AIR)
(From Reiter und Gabor, Zellt. und Str.)

The outcome was not materially different when a thin crystalline quartz plate was interposed between sender and detector. Gurwitsch (3) considered that these experiments proved the existence of a radiation, which he called mitogenetic rays.

YEAST AS A DETECTOR

Baron, a collaborator of Gurwitsch, found that yeast could also be used as a detector. He determined the increase in

the number of buds under definite conditions with and without the presence of the sender, and ascertained that, while the number of buds increased by 15 per cent without a sender, the increase was of the order of 25 per cent with a sender. Since in comparison with the onion, yeast is more easily handled, it is now generally used as a detector in place of onion root, which might now be said to have only an historical interest. The technique of this method is described by Gurwitsch (4) in Abderhalden's *Handbuch der biologischen Arbeitsmethoden*. In the same publication are to be found descriptions of the use of other detector materials, such as the corneal epithelium of frogs, Amphibia, etc.

using onion sole, muscle tissue, and cancer tissue as senders.

WHAT TISSUES EMIT RAYS?

Not only onion roots, but many other plant tissues and parts can be used as senders. Indeed Gurwitsch seems to be justified in speaking about the universal character of mitogenetic radiation. As an illustration, the following list of materials is presented, taken from the review article by Gurwitsch in *Protoplasma:* (1) Bacteria. The induction effect was first shown in these organisms by J. and M. Magrou with *Bacterium tumefaciens*, and later described by Baron, from Gurwitsch's laboratory, for different

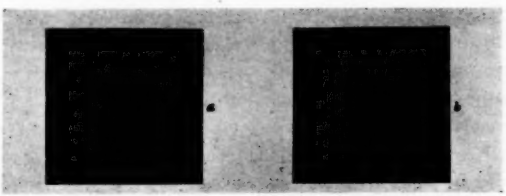

FIG. 2. MAKRO EFFECT WITH YEAST AFTER THREE DAY EXPOSURE
a = control; *b* = induced drop. (From M. Baron, Abd. H. d. biol. Arbm.)

According to Baron it is possible to produce the mitogenetic effect in a manner recognizable to the naked eye. He used a standard hanging drop containing about 8000 "old" yeast cells per cubic mm. in a moist chamber, which he closed with a thin quartz plate. Under this plate he placed a container with young rapidly growing yeast. A control chamber was prepared in identically the same manner, except that the quartz plate was replaced by a glass plate. The "macroeffect" shown in the accompanying figure became evident after three days.

The yeast technique has been tried with apparent success by the authors of the present article. Experiments were made

kinds of bacteria. (2) Yeast. The methods employing these organisms were first described by M. Baron, and later verified and used by Siebert. (3) Cleavage stages of animal eggs, in particular the animal pole of amphibian embryos in the morula stage (A. Anikin), and sea urchin eggs prior to cleavage (Frank and Salkind) as well as during the second and third cleavage stages (Salkind). (4) The yolk of the hen's egg during the first two days of incubation. Only the yolk from the subgerminal area was used, and this was found to lose its power to radiate after the beginning of the circulation. (5) Various portions of plant seedlings, especially of *Helianthus* (Frank and Salkind),

in particular the root tips (Rawin), the cotyledons, and plumule. (6) Leptom bundles of potato (Kisliak and Statkewitsch). (7) Fresh onion sole brei [The word "brei" means a pulp prepared by the grinding of tissue material.] (A. and L. Gurwitsch, Reiter and Gabor.) (8) Day old sterile beet brei (Anna Gurwitsch). (9) A brei made from young tadpole heads (A. Anikin, Reiter and Gabor). (10) Blood of frogs and rats (Gurwitsch, Sorin). (11) Corneal epithelium of starved rats (L. Gurwitsch). (12) Contracting muscle (Siebert, Frank). (13) Neoplasms (Gurwitsch, Siebert, Reiter and Gabor).

According to Gurwitsch it may be said in general that meristematic tissues do not give off rays, but that their mitogenetic impulse comes from other sources which themselves do not usually show cell division. Malignant cancerous tissue seems to form the only exception known to this rule; it is rich both in mitotic cells and in mitogenetic rays. Sea urchin eggs have the ability to radiate and to form mitotic figures, but in this case division and radiation have been shown to alternate. Gurwitsch mentions repeatedly that intense cell division and ability to radiate are not connected with each other.

THE WAVE LENGTHS OF MITOGENETIC RAYS

Any proof of the existence of the rays from a physical standpoint demands a study of their properties. As is evident from the experiments already described, a thin plate of crystalline quartz does not materially weaken their effect. But if this quartz plate is covered with a thin layer of gelatine the rays cannot penetrate. This definitely indicates that the rays must be short in wave length. They appear to show all the properties of light in the ultra-violet region. The rays can be reflected and they move in straight lines, as the accompanying figures indicate. Artificial sources (iron arcs, aluminium sparks, and the like) can also be used as senders. Frank, a coworker of Gurwitsch, has used an arc between aluminium electrodes as sender, before a large quartz spectrograph. In place of the photographic plate he used small blocks of agar covered with a fine layer of yeast cells. Each block covered a certain part of the spectrum, and he found that the greatest effects were evident in the wave length region of 200 to 240 mμ. He found further that an electrically irritated muscle which he used as sender before a small quartz spectrograph gave a distinctly greater increase of buds in the detector yeast culture between the wave lengths of 199 and 237 mμ than was observed at other wave lengths.

THE WORK OF REITER AND GABOR

The most important work on the subject of mitogenetic rays, aside from that of Gurwitsch and his students, is that of Reiter and Gábor (5). The authors last mentioned have recently published an excellent monograph on the subject, the product of three and a half years of experimental study. In the main their work substantiates that of Gurwitsch, but their methods of sectioning and counting are different. They used cross sections in place of longitudinal ones. In figure 4 a typical cross section is given, showing the effect of the radiation. The experimental set-up was the same as that of Gurwitsch. Like the latter author Reiter and Gabor come to the conclusion that a radiation occurs, but they believe its effect is to retard mitosis with the result that more cells are to be found in the mitotic stage at any one time than would be found in this state under ordinary conditions.

Like Gurwitsch they were unable to detect any radiation from narcotized roots,

but in contrast to his results they found that brei made of onion sole [short stem of onion bulb] radiates only when exposed to visible light. They verified the discovery of Gurwitsch that a brei made of the heads of tadpoles would radiate whereas body brei was inactive. In contrast to other investigations yeast and muscle were found ineffective; however, the experimental conditions were somewhat different. Many other tissues were likewise found to be inactive. Malignant tumors from fresh operative material as well as experimental malignant tumors gave the mitogenetic radiation, but benign tumors were ineffective. They found further, as had Gurwitsch and his coworkers, that the radiation can be reflected and broken up by refraction and travel in straight lines. In order to show that an external chemical influence was not the cause of the effect Reiter and

TABLE II

Number of agar block	1	2	3	4	5	6
Spectral region in mμ	186–199	199–219	220–240	240–270	280–320	320–360
Induction effect	−3.2%	+43%	+30%	+0.5%	+3.2%	−2%

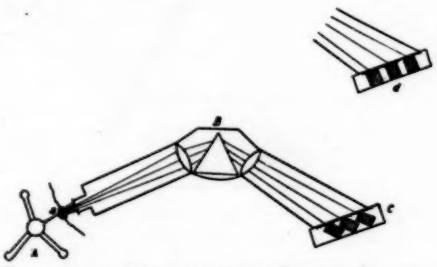

FIG. 3. SCHEMATIC VIEW OF EXPERIMENTAL ARRANGEMENT FOR SPECTRAL SEPARATION OF MITOGENETIC RADIATION OF A TETANIZED FROG-SARTORIUS

(*A*) Base with sartorius and connecting electrodes. (*B*) Schematic cross-section of spectroscope and path of rays. (*C*) Glass plate with marks showing the different wave lengths of spectrum; on glass plate are the agar blocks with yeast culture. They are arranged in the main figure so that radiation hits them vertically. In the side figure (*D*) with common front and oblique angle of radiation. (From G. Frank.)

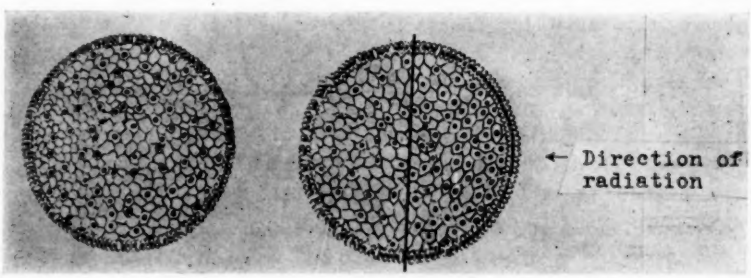

FIG. 4. SCHEME OF A NORMAL UNINFLUENCED AND OF AN INFLUENCED CROSS-SECTION OF A ROOT

The median line separates the half turned towards the sender and the one turned away. (From Reiter and Gabor.)

Gabor enclosed an onion root, with a bit of the bulb attached, in a quartz tube, which was then sealed and exposed to the sender; this experiment was successful.

In order to determine the wave length of the emitted rays Reiter and Gabor used a brei of onion sole as sender, placed before a specially built small quartz spectrograph. As detectors onion roots were used in place of photographic plates. They found that the emitted radiation fell in the neighborhood of 330 to 340 mμ. In other experiments they used artificial sources for the light, placing these before a large spectrograph, and found in contrast to Frank that the wave length 340 mμ gave the most favorable results, but that a slight increase in mitotic activity was observed also around 280 mμ. Between 290 and 320 mμ an antagonistic effect could be recognized.

The reason for the discrepancy in wave lengths found by Gurwitsch and those found by Reiter and Gabor may lie partly in the experimental technique. The latter authors used a much longer time of exposure than did Gurwitsch. The question, however, has by no means been cleared up as yet.

Inasmuch as the sunlight contains among others the wave-lengths around 340 mμ it might be expected to exert a favorable influence on mitotic activity, but since sunlight also contains the antagonistic wave lengths no beneficial effect is imparted. Reiter and Gabor have applied

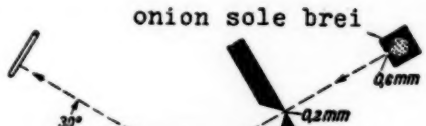

FIG. 5. REFRACTION OF MITOGENETIC RAYS IN WATER AND REFLECTION FROM MERCURY
(From Reiter und Gabor, Zellt. und Str.)

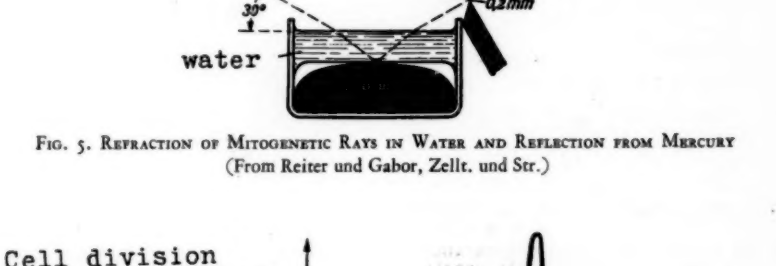

FIG. 6. DEPENDENCE OF INFLUENCE ON INDUCTION OF GROWING ZONE OF ONION-ROOT FROM THE WAVE LENGTH
(From Reiter und Gabor, Zellt. und Str.)

the results of their investigations to therapeutic treatments.

THE WORK OF OTHER INVESTIGATORS

In Paris, J. and M. Magrou (6), working independently of the Gurwitsch school and experimenting on *Bacterium tumefaciens* and cancerous tissue, reported verification of Gurwitsch's results.

Siebert (7) was the first to find that working muscles gave out the rays while muscles at rest did not. He worked not only with whole muscles but with muscle brei. He tried to repeat the experiments of Warburg (8) and reported success. According to his belief the rays emanating from muscle originate in an oxidation process. In his later experiments Siebert used yeast as a detector, the effect being recognized, following the method of Baron, by a higher percentage of yeast buds in the exposed culture as against that in the unexposed control. He observed an increase also in the areas adjacent to the actual paths of the rays. He believes that, associated with the Gurwitsch mitogenetic radiation there is a so-called "*Aktionsstrahlung.*"

Stempell (9), a German investigator, found that the decomposition of hydrogen peroxide could be accelerated when it was exposed to the radiation from onion brei. Furthermore he obtained a most striking proof of the effect of mitogenetic rays on the formation of Liesegang rings. This has since been discredited.

The experiments of Gurwitsch with onion root tips and yeast have also been verified in an article which has just appeared in this country (10).

PHOTOGRAPHIC METHODS

Many authors have sought to obtain proof of these rays by means of the photographic plate, but with the exception of one very questionable case they have been unsuccessful. Gurwitsch believes that the photographic method is the least promising. Common photographic plates are not very sensitive to the radiation around 220 mμ. They might perhaps be sensitized by special treatment or Schuman plates might be used; but all these plates are less sensitive for the wave-lengths concerned than are the most sensitive plates to longer wave-lengths. It is quite possible that the rays may exist and at the same time not be detectable by a photographic plate. Researches in the laboratory of the Eastman Kodak Company have shown that several hundred absorbed light quanta are necessary to make a single grain developable. It is very possible that the number of quanta in mitogenetic rays is not sufficient to give this result. According to calculations carried out in Gurwitsch's laboratory the onion root is 600 times as sensitive as the photographic plate. It seems therefore that it will be necessary to develop a much more sensitive means of detecting rays if such very weak light phenomena are to be properly investigated. Such a detector would open up a whole new field in photochemistry. Experiments along this line are at present being carried out by Gurwitsch and his co-workers.

Of the investigations that have failed to verify the results of Gurwitsch the work of Rossmann (11) is the most outstanding. Rossmann believes that the criterion of counting mitoses is not a reliable one, and that the exact number of cell divisions in process cannot be determined with any degree of accuracy. Since Rossmann's attack Gurwitsch has published more detailed procedures, and an abundance of statistical material supporting his theory. Gurwitsch claims also that Rossmann has not repeated his experimental conditions and has also used plant roots as indicators

that were not as well suited to the purpose as is the onion root.

Gurwitsch compares the causes of the mitogenetic radiation with the causes of the radiation of fireflies, after the classical experiments of Dubois.

Another interesting bit of experimental work on onion sole brei deserves mention. If a sample of brei is kept for forty minutes or longer it ceases to give out the mitogenetic rays. Furthermore if fresh onion brei be heated to 56°C. it also ceases to radiate. However, if these two samples be stirred up together the mixture gives off radiations. In analogy with the terms used in the study of bioluminescence the two substances in the brei have been called by Gurwitsch *mitotin* and *mitotase*. These substances are assumed bodies, which have not yet been separated.

About the cause of the mitogenetic radiation of yeast and bacteria nothing is known. More is known about the radiations from animals. The results obtained from the study of blood radiation are not clear, and can hardly be adequately explained at the present stage of research. Blood serum does not of itself give off radiations, but by the addition of oxyhaemoglobin or hydrogen peroxide it can be made to do so, which seems to indicate an oxidation reaction. While the blood of normal healthy rats gives off radiations, the blood of starved rats does not, but if a little glucose be added to the latter radiations are detectable.

If one accepts the effect as established there remain the various explanations of it which have been suggested. The solution of the difficulty which appeals to a chemist would be to postulate the existence of a reaction, probably an oxidation, which emits light during its progress. The unfortunate experience of the chemist with the radiation theory of chemical reaction would, however, teach him to be more than careful in accepting such an explanation without proof beyond reasonable doubt.

The problem is without doubt one of the most intriguing of the many scientific problems before us today. It is one in which workers both in biological and physical sciences may well be interested.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

LA VARIATION ET L'ÉVOLUTION. *Tome II. L'Évolution.*
By Émile Guyénot. *G. Doin et Cie.*
32 francs (paper) *Paris*
4¼ x 7; viii + 414

The first part of this book, dealing with variation, has already been reviewed in these columns. With regard to evolution, Guyénot concludes that there is no good evidence of the inheritance of acquired adaptations, that mutations furnish the only basis for evolution of which there is actual evidence. However, observed mutations have always been in the details rather than in the fundamental plan of organisms. "It is impossible for us to imagine by what mechanism the different types of organization which characterise the major zoological groups can have been formed, can have been derived from one another or from ancestral forms. On this point the paleontological documents are silent." This is much like the conclusion that Clark reaches in *The New Evolution*. However, Guyénot, more cautious than Clark, does not go on to postulate that life at its very beginnings from the single cell developed simultaneously and at once in every possible direction. To us this agnostic attitude seems the wiser.

ANIMAL ECOLOGY AND EVOLUTION.
By Charles Elton. *Oxford University Press*
$1.50 4¾ x 7¼; 96 *New York*

This little book merits the careful consideration of students of biology. The three essays, the substance of lectures delivered in the University of London on the future of animal ecology are as follows: The regulation of numbers; the significance of migrations; and the real life of animals. It is the author's opinion that

> the study of the relations between wild animals and their surroundings cannot be properly conducted with a background of theory or hypothesis on the subject of evolution, that the present remarkably chaotic state of the largely unco-ordinated data that animal ecologists have collected is mainly to be attributed to the lack among biologists of an adequate philosophy of animal life, and, finally, that the time is now ripe for some such philosophy to be formulated, if only in a very tentative and preliminary form.

A STUDY OF THE PHYTOSAURS *with Description of New Material from Western North America.*
By Charles L. Camp.
University of California Press
$3.50 10 x 13; x + 172 (paper) *Berkeley*

This report deals with a "series of 'species' of phytosaurian reptiles from a number of stratigraphic horizons in the Chinle Triassic, with comparisons between

these forms and forms previously described, and with the structure of the phytosaur skeleton." The author contributes new data on the American Mesozoic "Red Beds" and presents a "tentative classification and phylogeny of the phytosaurs, with a discussion of their relationships." Included in the work is a map showing a distribution of Chinle Triassic formation as exposed in Nevada, Arizona, Utah and Western New Mexico, also drawings, figures and plates, a literature list of 100 titles and an index.

THE PLEISTOCENE OF NORTHERN KENTUCKY. *A Regional Reconnaissance Study of the Physical Effects of Glaciation within the Commonwealth. Presented with Four Separate Geological Papers by Stephen Sargent Visher, Arle H. Sutton, James K. Roberts and Armin Kohl Lobeck.*

By Frank Leverett.

The Kentucky Geological Survey

$1.25 $5\frac{5}{8}$ x $8\frac{3}{4}$; xi + 403 *Frankfort*

Five papers on the geology, physiography, and climate of Kentucky.

EVOLUTION.

By E. W. MacBride.

Jonathan Cape and Harrison Smith

60 cents $4\frac{1}{2}$ x $6\frac{3}{4}$; 122 *New York*

A brief exposition for the layman of some of Prof. MacBride's ideas concerning evolution, which are already well known to biologists.

GENETICS

THE GENETICS OF DOMESTIC RABBITS. *A Manual for Students of Mammalian Genetics and an Aid to Rabbit Breeders and Fur Farmers.*

By W. E. Castle. *Harvard University Press*

$1.25 *Cambridge*

6 x 9; vi + 31 + 13 plates.

Students of genetics and rabbit breeders will be interested in this manual. It is designed to aid students of genetics to "understand better what rabbit breeders of this and previous centuries have accomplished and how they have been able to accomplish it." The rabbit breeder will better understand "the genetic constitution of his animals and so be able to alter that constitution to suit his purposes." The work includes a table showing the genetic constitution of various breeds or rabbits, a list of references and a group of plates.

PRINCIPLES OF DOG BREEDING. *A Presentation of Heredity in Dogs, the Anatomy and Functioning of the Sexual Organs, and the Selection of Bloodlines.*

By Will Judy. *Judy Publishing Co.*

$2.00 $5\frac{5}{8}$ x $8\frac{1}{2}$; 118 *Chicago*

There are several points to which the geneticist would object, for example the definition of mutation as "the appearance in the offspring of a quality not possessed by either parent but considered to be a quality possessed by previous ancestors. This quality newly appearing may be transmitted," and the Bruce Lowe theory of saturation, which holds that the "dam, with each successive mating by the same sire, absorbs some of the nature of the circulation of the unborn offspring until after a time she becomes saturated with the sire's blood or nature." However, these theories do not seriously mar the practical value of the book for practical breeders. There is much sound advice given which the author has gleaned from long experience in dog breeding. The book is well illustrated. There is no index.

GENETICS AND EUGENICS. *A Text-Book for Students of Biology and a Reference Book for Animal and Plant Breeders. Fourth Revised Edition.*
By W. E. Castle.
Harvard University Press
$3.00 5¾ x 9; x + 474 *Cambridge*

In this edition of one of the best text-books on genetics a fuller account is given of human heredity; the chapter on the unit characters of rodents, on which much of our knowledge of mammalian heredity depends, has also been rewritten, as well as the discussion of polyploidy, parthenogenesis, and the artificial production of mutations. Dr. Castle has the enviable faculty of presenting his subject in a clear and readable form without writing down to his readers. Perhaps that is because he really knows it himself.

PRACTICAL APPLICATIONS OF HEREDITY.
By Paul Popenoe.
The Williams and Wilkins Co.
$1.00 5 x 7⅝; ix + 128 *Baltimore*

A careful reading of this book fails to disclose the relevance of the title. Actually it is a collection of chatty essays on subjects connected with heredity, containing some amusing material, and a great deal of doubtful logic and uncritical acceptance of almost anything that fits in to make a point. A sample:

"There are a few cases where a child of superior parents is set adrift for some unusual reason. Moses is the classical example."

LA FORMATION DE L'ÊTRE. *Histoire des Idées sur la Génération.*
By Jean Rostand. *Librairie Hachette*
12 francs 4¾ x 7½; 222 (paper) *Paris*

This excellent little book traces the history of genetics and embryology from antiquity to the present day. There is a bibliography and an index.

LOCALIZAÇÃO DOS FACTORES NA LININA NUCLEAR COMO BASE DE UMA NOVA THEORIA SOBRE A HEREDITARIEDADE.
By S. de Toledo Piza Junior. *Piracicaba*
6⅜ x 9; 98 (paper) *São Paulo, Brazil*

In this book the author gives his reasons for concluding that the hereditary factors are carried, not in the chromatin, but in the linin, and that they are not linearly distributed.

GENERAL BIOLOGY

INTRODUCTION À LA BIOLOGIE EXPÉRIMENTALE. *Les Êtres Organisés, Activités, Instincts, Structures. Encyclopédie Biologique VIII.*
By Paul Vignon. *Paul Lechevalier*
Paris
210 francs 6½ x 9⅞; 731 + 890 figs. + 24 plates (in colors) (paper)

If Plato and Aristotle in the early nineties had been awakened from their long sleep in the world-beyond, trained at the Sorbonne by Delage and Hérouard in animal morphology, especially protozoology and entomology, in drawing and painting by skilful masters, and then had devoted several years to morphological study of tropical leaf insects at Professor Bouvier's laboratory at the Muséum, they might have written this excellent book, and it would have been much to their credit. But special conditions would have been necessary: no contamination with Descartes' or Darwin's mechanistic heresies, no courses in biochemistry or biophysics. Twentieth century notions of matter, its electrons and protons, and of biological

mutations, being sufficiently idealistic, would have been permitted.

Plato would have collaborated chiefly by writing the numerous summaries and putting the book into its excellent form, with its several indices, biological and philosophical. Aristotle would have gathered material from museums far and wide, and carefully selected from the works of many modern biologists such results as lend themselves to idealism. He might perhaps have prepared the numerous fine illustrations and the colored plates of protectively-colored orthoptera.

Here we find animal behavior as viewed by Koehler, Jennings and Mast, who see things whole, rather than by such scapegrace analysts as Jacques Loeb, who is ignored, and Bohn, whose writings are listed but passed over in silence. We find a wealth of well-presented cases of psychic and infra-psychic behavior of protozoa, insects, birds and mammals; an array of beautiful forms in Radiolaria and other Protozoa are carefully described; many examples of protective resemblance are depicted with scientific accuracy and thoroughly discussed, chiefly from the standpoint of artist and poet. In brief, organisms are ideas; each has within itself, from the egg onward, its own inscrutable perfecting principle, directing its development toward utility and beauty.

Interesting examples of evolution by mutation are described in detail; natural selection no longer has a monopoly in the production of adaptations.

THE NATURE OF LIVING MATTER. *By Lancelot Hogben. Alfred A. Knopf, Inc.* $5.00 5½ x 8⅝; ix + 316 *New York*

A series of well-written essays contributing to the current discussion of some of the philosophical problems of biology. The twelve chapters fall into three groups dealing respectively with vitalism and mechanism; Darwinism and the atomistic interpretation of inheritance; and holism and the publicist standpoint in philosophy. The first section makes a slighter contribution in respect of either novelty or profundity than either of the other two, though it is necessary to get early into the record the author's neo-mechanistic position and its bases. Part II delivers some sound and penetrating thrusts to the uplifters who endeavor to base their philosophy and mode of life upon biology. Hogben's main thesis is that biology has no more to do with ethical problems than does physics or algebra. As a science biology is ethically neutral. The following remarks (pp. 214–215) about eugenics have a refreshing *Klang*.

> I believe that the eugenists have performed a useful task in emphasizing the need for a biological analysis of human society. The furtherance of that task will not be promoted by propaganda which overstates the achievements of the present, while underestimating the difficulties which lie ahead. Evolutionary inquiry was brought to an end in ancient Greece, when philosophy became the handmaiden of politics. Further progress was checked when philosophy became the bondservant of theology. Eugenics like Greek philosophy derived its first impulse from natural science. It soon entered into alliance with the politician. It is fast finding its most stalwart supporters among the clergy. It can only realize the aims of its founder by bringing the science of genetics into closer relationship with other methods of studying human biology and annulling the marriage of biological inquiry with political propaganda. As a private citizen the biologist is entitled to his own opinions concerning the merits of sterilizing the unfit, just as he is entitled to his own opinions on the Single tax or the advantages of capital punishment. Such opinions usually belong to his private world. In his public capacity, as a biologist, he is primarily concerned with sterilizing the instruments of research before undertaking surgical operations upon the body politic.

This book should be widely read, if for no other reason than to serve in some degree as an antidote to the sickly mys-

ticism now emitted in such great volume by eminent physicists. Unfortunately there is no index, nor a single line of bibliographic documentation. If the general reader, at whom this clever and useful book is aimed, should by any chance want to look for himself at the writings of the considerable number of persons not approved of by the author, he must do so strictly under his own steam, so far as Professor Hogben is concerned. Perhaps this is a part of the "publicist" philosophy.

THE CREED OF A BIOLOGIST. *A Biological Philosophy of Life.*
By Aldred S. Warthin. *Paul B. Hoeber, Inc.*
$1.50 5 x 7½; viii + 61 *New York*

This essay was written in anticipation of the retirement of its distinguished author from active teaching. It is dedicated to those of his "old students who understood." The text is a development and *apologia* of the following *Credo:*

I Believe in the Law

In the immortality of the germ plasm and in the creative progressive evolution of life, in the variability of value of the germ plasm through heredity and environment, in the transmission of acquired characters and in the conscious improvement of the race through the laws of volative eugenics. I believe that the aim of the individual life is the protection, improvement and continuation of the immortal germ plasm, and that this is best secured by self-development in the highest possible degree through a permanent monogamic sex-partnership with limitation of offspring towards the securing of the best possible results in the progeny, and their best preparation for the continuation of the process in the next generation. In this belief the Universe is rationalized for my intelligence and reason. I accept it with optimism, relinquishing all desire for a personal immortality, and unafraid believing that whatever gods may be, the game of life will have been played squarely and according to the Law.

All biologists, not to speak of other honest folk, will not agree with all of this creed. Perhaps it is because they do not "understand."

THE NATURE OF LIFE.
By Eugenio Rignano.
Harcourt, Brace and Co.
$2.75 5½ x 8½; x + 168 *New York*

In this volume of the *International Library of Psychology, Philosophy and Scientific Method* Professor Rignano further develops his solution of a problem which has vexed biologists since the days of Aristotle, the problem of the purposive aspects of living organisms, on which is based the war between mechanism and vitalism. The physico-chemical investigation of the mechanism of living processes, he finds, does not give a satisfactory explanation of their purposive character. Even if we had experimental evidence that the development of the mammary glands during pregnancy is caused by a hormone, this would in no way explain why the hormone sets in action a process which will be of use to the child about to be born rather than some useless process. On the other hand, the animistic vitalism of such writers as Driesch and Bergson is even more impotent in face of this problem. The entelechy and the *élan vital* are empty forms of words, explaining nothing.

With these criticisms we have much sympathy, but we are not sure that Professor Rignano's own solution really solves the problem. He wishes to postulate a new form of energy in living organisms, distinct from the forms of energy familiar in inorganic systems. It is not entirely clear to us, however, that this nervous energy is better adapted to explain the purposiveness of life than our old friends, physical and chemical energy. Perhaps we do Professor Rignano an injustice. His style, as is no doubt fitting in a philosopher, often resembles that of the Duchess in *Alice in Wonderland.* Possibly if the book were translated into a tongue understanded of the people, the superiority of nervous energy to its elder brothers might

be clear. Meanwhile we sympathize with Lotka.

THE CONQUEST OF LIFE.

By Theodore Koppanyi. D. Appleton and Co.
$2.00 5 x 7¾; xii + 263 *New York*

Another popular account of what biology is all about. It attracted us but little. One or two passages will perhaps help the reader to understand our lukewarmness.

The doctrine of mimicry should be discarded. There exist neither models nor imitators, and there is no real protection by imitation.

Are we to infer that these external factors affect only one generation and cannot produce any changes in the progeny? By no means. First of all we can assume very easily that the same external factors which influence the first generation will continue to influence the second, the third, the fourth and the later generations, provided they live in the same environment. Perhaps, if the external factor influences many generations one after the other it sooner or later will affect the germ cells too. After all, such a powerful external factor as light, heat, or electricity can easily modify not only the cells of the body surface, but also the germ cells, which lie rather deep in the body. It is here that we hit upon one of the real causes of evolution, namely, that external factors produce sudden changes in the make-up of the germ cells within the animal.

As a result of his studies, Dr. Kamm is able to classify individuals as "wet" or "dry." This is what he says:

"Some individuals, the physiological wets, are extremely sensitive to the action of the Beta hormone. Others react readily to normal administration of the hormone, and they are the physiological drys.

"The fleshy type of individual is almost invariably the wet type, whereas the slender, scrawny individual is usually dry. The suggestion is therefore made that we have here possibly one of the important explanations why the former is fleshy and why the latter fails to put on weight readily in spite of an excessive taking of food and water.

"It is apparent that the portly person who is desirous of reducing must cut down on his liquid intake, as well as his intake of solid food. As for the scrawny person, gland therapy may possibly be indicated; but while the work is still in the investigative stage, conclusions cannot be drawn."

Sex life, then, when viewed from Olympian heights is perhaps the most unselfish, the most altruistic function of any being. Some insects mate, and once this life work is completed, are ready to die within a few hours.

THE HISTORY OF BIOLOGICAL THEORIES.

By Emanuel Rádl (Translated and adapted from the German by E. J. Hatfield).
Oxford University Press
$6.00 5½ x 8½; xii + 408 *New York*

A translation of the greater part of the second volume of the original German, with unimportant revisions and omissions. The book seems to us of slight merit. As history, it rates far below Nordenskiöld's and as philosophy it makes no appeal, at least to us. The author was formerly a biologist, who worked on tropisms and on the structure of the nervous system; he has turned philosopher, and now professes natural philosophy at Prague. His career is thus reminiscent of that of Driesch (he is, like Driesch, a vitalist), but he is a much less important and less interesting worker. The book is mildly interesting.

PRINCIPLES OF ANIMAL BIOLOGY.

By Lancelot T. Hogben. Christophers
8 s. 6 d. net 5 x 7⅝; xxiv + 332 *London*

A sound elementary text book, which tries to minimize technical terms, and to emphasize principles. This is, of course, what every up-to-date text-book writer says of his own work; but in the present case the effort has been rather more successful than sometimes happens.

COLLEGE BIOLOGY.

By Henry R. Barrows.
Richard R. Smith, Inc.
$3.00 5½ x 8½; xv + 414 *New York*

Precision and clarity are great virtues

in a writer. They are, unfortunately, not always in evidence in this book.

It has been demonstrated by many breeding experiments, particularly with the fruit fly, that new characters known as mutants, not infrequently appear. Various new species have been produced in Drosophila which, though derived from the same ancestor, are sterile with respect to one another. This is a clear indication that in Drosphila, at least, new species can arise by mutation.

The figures are stated to have all been redrawn from the originals. This we regard as in many cases unfortunate.

OLD AGE. *The Major Involution. The Physiology and Pathology of the Aging Process.*
By Alfred S. Warthin. Paul B. Hoeber, Inc.
$3.00 5¼ x 8¼; xvi + 199 *New York*

This is an expansion of a lecture on the pathology of the aging process delivered before the New York Academy of Medicine in 1928. It is interestingly written and well illustrated. The theory of senescence which Warthin espouses, which is not novel, is that

age, the major involution, is due primarily to the gradually weakening energy charge set in action by the moment of fertilization, and is dependent upon the potential fulfullment of function by the organism. The immortality of the germ plasm rests upon the renewal of this energy charge from generation to generation.

There is an index, but no bibliography.

CONTRIBUTIONS TO MARINE BIOLOGY. *Lectures and Symposia Given at the Hopkins Marine Station, December 20-21, 1929, at the Midwinter Meeting of the Western Society of Naturalists.*
Stanford University Press
$7.50 *Stanford University*
6⅝ x 10; viii + 277

Twenty-three papers read before the Western Society of Naturalists in December, 1929. They are grouped under the general headings oceanography, permeability, photosynthesis, early development, marine algae, and growth. The opening paper by Prof. C. A. Kofoid on the evolution of the Tintinnoinea is a particularly interesting and valuable contribution.

COULEURS ET PIGMENTS DES ÊTRES VIVANTS.
By Jean Verne. *Armand Colin*
12 francs (cloth) *Paris*
10.50 francs (paper)
4⅜ x 6¾; 219

A careful study of the chemistry and physiology of animal and vegetable pigments.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 329. Weitere Fortschritte in der Züchtung von Warmblütergewebezellen in vitro.*
By Albert Fischer and Hans Laser.
Urban und Schwarzenberg
3.50 marks 7 x 10; 44 (paper) *Berlin*

Completes Part I of Section V with a description of the most recent advances in the technique of tissue culture.

THE BIOLOGY STUDENT.
Edited by Marguerite F. Little.
Science Publishers, Inc.
1386 W. North Ave., Baltimore
Individual subscriptions, $2.00 for 18 issues
Ten or more to one address, $1.00 each for 18 issues

The first number of an entertaining little nature-study magazine for school children. We wish it all success.

HUMAN BIOLOGY

THE NEGRO IN AMERICAN CIVILIZATION. *A Study of Negro Life and Race Relations in the Light of Social Research.* *By Charles S. Johnson.* *Henry Holt and Co.* $4.00 5½ x 8½; xiv + 538 *New York*

In order that there might be available accurate information concerning the Negro and his status in the United States there was organized several years ago a central executive committee from sixteen national organizations which were interested in the race problem. In 1928, there was held in Washington a National Interracial Conference for the purpose of receiving the reports of the investigators and studies which had accumulated as a result of the committee's work. This book is the result of the Washington conference. Mary Van Kleeck says in the foreword that "it is neither the proceedings of the conference nor the report of an investigation. It is a synthesis of many studies put through the process of a conference which hammered it into coherence and reality." To Dr. Charles S. Johnson fell the task of gathering the material for publication.

The volume is in two parts. In the first section are discussed under twenty-five headings many of the problems of the economic and social status of the negro; his health, causes of death, hospitalization, schools, juvenile delinquency, citizenship rights, etc. Part II contains papers written for the conference. These are as follows: "The Negro in Industry", by Niles Carpenter; "The Health of the Negro", by Louis I. Dublin; "Biological Factors in Negro Mortality", by Raymond Pearl; "The Negro and the Problem of Law Observance and Administration in the Light of Social Research", by Thorsten Sellin; "The Negro Citizen", by W. E. B. Du Bois; "Race Relations", by Herbert A. Miller. There is included in these various chapters questions which came up for discussion and summaries of the discussions.

Much labor has gone into the preparation of the volume and the records which it gives will be of service in future work. An immense amount of statistical material is presented especially in the first section of the book. There has been little attempt at this time to find remedies for many of the existing evils. Included in the work is a lengthy bibliography, a list of the organizations and committees responsible for the conference, a program of the meetings and an excellent index.

TWINS. *Heredity and Environment.* *By Nathaniel D. Mttron Hirsch.* *Harvard University Press* $2.00 5½ x 8¾; 159 *Cambridge*

In this study, three groups of twins were chosen; (a) fifty-eight pairs of dissimilar twins living under similar environments; (b) thirty-eight pairs of similar twins living in similar environments and (c) twelve pairs of similar twins living in dissimilar environments. In all cases the twins were like-sexed. Statistics, which are given in tabular form in the book, were collected on anthropometric measurements, disease, history, hand writing, tests of manual and motor ability, educational and intelligence tests, etc. Among the conclusions that the author reaches are the following:

> The study demonstrates that heredity and environment both contribute to the intelligence and anthropomorphic qualities of the individual, but that their contributions are far from being equal. Neither the extreme hereditist nor the extreme environmentalist is correct, but the contribution of heredity is several times as important as that of environment.
>
> Heredity and environment vary in their relative importance in relation to specific or general traits.
>
> Education and training vary in their influence in

proportion to the hereditary type with which they are dealing—the more intelligent the individual the more potent educational and general environmental influence.

This is an important contribution. It is written in as non-technical language as possible in order to interest the layman as well as the specialist.

GROWING UP IN NEW GUINEA. *A Comparative Study of Primitive Education.* *By Margaret Mead.* *William Morrow and Co.*
$3.50 5¼ x 8; x + 372 *New York*

During six months of field work on the Manus Islands, Dr. Mead lived intimately with the native tribes, and acquired an excellent understanding of the development of the Manus child into an adult. She watched the baby, the child, the adolescent with a view to finding out the manner in which each one of these periods contributes towards the making of an adult. From babyhood until the near completion of adolescence, children are kings; the only demands made upon them being those of physical training. Each infant early learns to assume responsibility for its own welfare, a necessity if it is to survive a precarious mode of life, where all dwellings are flimsily "constructed over incoming and outgoing tides of the lagoon"; and adults are too busy gathering sustenance to keep watch over every movement of the child. At a remarkably early age the child has acquired great agility and nearly perfect coordination. Left to themselves the children romp all day long with no regard for discipline.

The children are allowed to give their emotions free play; they are taught to bridle neither their tongues nor their tempers. They are taught no respect for their parents; they are given no pride in their tradition. The absence of any training which fits them to accept graciously the burden of their tradition, to assume proudly the rôle of adults, is conspicuous. They are permitted to frolic in their ideal playground without responsibilities and without according either thanks or honour to those whose unremitting labour makes their long years of play possible.

Among these people, however, it does not happen, as some modern educational psychologists would have us believe, that the child allowed such freedom develops a richly creative spirit. The children merely play like pups, stopping only to rest so that they may play again.

In the second part of the book, the author makes pertinent comparisons between Manus and American Societies. She finds there is much similarity. The Manus peoples make their chief concern the amassing of property, enforcing morality, (quite as rigid as our most strict Puritanism) and security for the next generation.

There is . . . . a curious analogy between Manus society and America. Like America, Manus has not yet turned from the primary business of making a living to the less immediate interest of the conduct of life as an art.

Marriage, maturity, and middle age are not looked forward to with any joy, for they mean only hard work to pay off debts and keep living.

Above the 35-year-olds comes a divided group—the failures still weak and dependent, and the successes who dare again to indulge in the violence of childhood, who stamp and scream at their debtors, and give way to uncontrolled hysterical rage when crossed.

This study in comparative anthropology is keen, and deserves consideration.

THE STORY OF PUNISHMENT. *A Record of Man's Inhumanity to Man.* *By Harry E. Barnes.* *The Stratford Co.*
$3.00 5½ x 8½; vii + 292 *Boston*

This book will be of interest to two classes of readers; first, sentimentalists and others who believe in the uplift; and second, bloody-minded, hard-boiled eggs

who enjoy prize fights and bull-baiting. The author gives a fair and adequate account of the various devices which have been invented for the annoyance of the convicted criminal, and a gruesome lot they are. The historical part of the book is, we think, much better than the hortatory. When Professor Barnes begins to argue as to what ought to be done to suppress crime, his logic becomes somewhat shaky, and his faith in "scientific criminology" leads him into what we can only describe as roseate optimism as to what might be accomplished. We have no quarrel with his ideals; it would be indeed a pleasanter world if Professor Barnes would reorganize the whole judicial and penal system and make it work; but we remain dubious as to whether it really would work. Also, as a taxpayer, how much would it cost? Of course, we shall be told that it would really save money, because so many criminals would be turned into valuable and productive citizens. Maybe so; but our observation is that in such reforms the taxes are paid in real money, and the savings are paid in the currency of the Musical Banks of Erewhon.

CHILDREN OF THE COVERED WAGON. *Report of the Commonwealth Fund Child Health Demonstration in Marion County, Oregon, 1925–1929.*
By Estella F. Warner and Geddes Smith.
The Commonwealth Fund Division of Publications
$1.00 postpaid 6 x 9; 123 *New York*

A CHAPTER OF CHILD HEALTH. *Report of the Commonwealth Fund Child Health Demonstration in Clarke County and Athens, Georgia, 1924–1928.*
The Commonwealth Fund Division of Publications
$1.00 postpaid 6 x 9; v + 169 *New York*

FIVE YEARS IN FARGO. *Report of the Commonwealth Fund Child Health Demonstration in Fargo, North Dakota, 1923–27.*
The Commonwealth Fund Division of Publications
$1.00 postpaid 6 x 9; 207 *New York*

CROSS-SECTIONS OF RURAL HEALTH PROGRESS. *Report of the Commonwealth Fund Child Health Demonstration in Rutherford County, Tennessee, 1924–1928.*
By Harry S. Mustard.
The Commonwealth Fund Division of Publications
$1.00 postpaid *New York*
6 x 9; xiii + 230

Four attractively presented accounts of the Commonwealth Fund's health demonstrations in Oregon, Georgia, Tennessee, and North Dakota. As public health propaganda they are excellent. To our low commercial mind they are a trifle unsatisfying, because we are unable to find any very convincing evidence of tangible results. This is not to say that the demonstrations accomplished nothing; but merely (as is carefully stressed in each of these volumes) to point out that there is no satisfactory method of measuring what was accomplished.

FIFTY-TWO YEARS OF RESEARCH, OBSERVATION AND PUBLICATION 1877-1929. *A Life Adventure in Breadth and Depth. (With Complete Bibliography Chronologic and Classified by Subject 1877–1929.)*
By Henry F. Osborn. Charles Scribner's Sons
$1.50 5¾ x 8⅞; 160 *New York*

An autobiographic bibliography. The extent of Professor Osborn's labors and interests may be gauged by the fact that the chronological bibliography closes with number 801 (and there are more than 801 titles); and the classification by subject gives us the following:

Geology	12
Correlation, Zoogeography	10
Palaeontology	295
Zoology	31
Odontology and Odontography	11
Embryology	4
Neurology	9
Psychology	5
Anthropology	55
Evolution (Biology, Religion)	81
Eugenics (Heredity)	8
Education	58
Administration	78
Conservation	8
Biography	104
Miscellaneous	34

ILLUSTRATIONS AND PROOFS OF THE PRINCIPLES OF POPULATION. *Being the First Work on Population in the English Language Recommending Birth Control. Now Exactly Reproduced. With an Introduction Demonstrating Francis Place as the Founder of the Modern Birth Control Movement, Together with Unpublished Letters of Place on Birth Control, Coleridge's Criticisms of Malthus' Views on Birth Control.*

By Francis Place. Critical and Textual Notes by Norman E. Himes.

Houghton Mifflin Co.
$4.50 *Boston and New York*
$5\frac{1}{2}$ x $8\frac{5}{8}$; 63 + xv + 355

Lowell, who liked

> An old fashioned title-page, such as presents
> A tabular view of the volume's contents,

would have been delighted with the title-page of this book, a photographic reproduction of an important but rare work. Place, while agreeing with Malthus as to the necessity for controlling the increase of population if the condition of the working classes were to be improved, concluded that Malthus' remedy of long-delayed marriage was impracticable and that

> If, above all, it were once clearly understood, that it was not disreputable for married persons to avail themselves of such precautionary means as would, without being injurious to health, or destructive of female delicacy, prevent conception, a sufficient check might at once be given to the increase of population beyond the means of subsistence; vice and misery, to a prodigious extent, might be removed from society, and the object of Mr. Malthus, Mr. Godwin, and of every philanthropic person, be promoted, by the increase of comfort, of intelligence, and of moral conduct, in the mass of the population.

SOIL. *Its Influence on the History of the United States. With Special Reference to Migration and the Scientific Study of Local History.*

By Archer B. Hulbert.

Yale University Press
$2.50 6 x 9; xii + 227 *New Haven*

The writing of history has changed greatly since the days when it was merely a chronicle of wars and other political events. In spite of the jeremiads of die-hard historians of the old school, it has come to be recognized that politics are explicable only by considering the social and economic factors which underlie them, and that these in turn can be understood only in relation to man's environment. However, in this interpretation it has been mainly the topographic factors which have been emphasized, such as the influence of rivers and their valleys as means of communication and that of mountains as barriers. In this interesting book Mr. Hulbert takes up the hitherto neglected topic of the influence of soils on American settlement and expansion. As he points out, this is a subject which needs to be worked out in detail by local historians. The economic interpretation of history, although in some cases perhaps overworked, has been rich in results; we look forward to as abundant a harvest from its geological and biological interpretation.

PHYSICAL DIAGNOSIS.
By Warren P. Elmer and W. D. Rose.
The C. V. Mosby Co.
$10.00 6 x 9; 903 *St. Louis*

Owing to the death of the author of this book Dr. Elmer was requested to revise the sixth edition. While much of the original subject matter has been retained there has been considerable change made in arrangement, a good deal of rewriting done and additions made to carry out the revisor's ideas. The book is now divided into two parts. Part I deals with the technic of physical examination and normal physical diagnosis; Part II with the physical diagnosis of disease. While designed primarily as a text-book for medical students, there are many aspects of this book which will be of great value to the physical anthropologist.

THE MEASUREMENT OF MAN.
By J. A. Harris, C. M. Jackson, D. G. Paterson, R. E. Scammon.
University of Minnesota Press
$2.50 6 x 9⅛; vii + 215 *Minneapolis*

Four lectures delivered under the auspices of Sigma Xi at the University of Minnesota. The late Professor Harris contributes illustrations of the various statistical techniques applicable to mass measurements. Professor Jackson writes on normal and abnormal human types; he is chiefly concerned with variation and correlation in body measurements. Professor Paterson, dealing with personality and physique, reviews the present state of our knowledge, with much valuable critical comment on the work that has been done. Professor Scammon, in the final paper, deals with the measurement of growth, and especially with the growth of the various parts and organs. We found this the most interesting chapter of the book.

SEVENTY BIRTH CONTROL CLINICS.
A Survey and Analysis Including the General Effects of Control on Size and Quality of Population.
By Caroline H. Robinson.
The Williams and Wilkins Co.
$4.00 5⅝ x 8⅜; xx + 351 *Baltimore*

Mrs. Robinson's book is intended to be a practical hand-book for those interested in the birth control movement. She has made a general survey of seventy organized centers located in the following countries: Germany, 16; Austria, 8; Russia, 1; Great Britain, 14; and the United States, 31. These furnish information on the

nature, extent, and status of this world-wide movement, insofar as these may be objectively determined by descriptions of leaders and workers, their policies, procedures, and experiences, the numbers of people reached, the expenses entailed, and the character and trend of public and professional opinion as reflected in laws and letters.

The book is in two parts; the first contains six chapters dealing with the survey, while the four chapters in the second part are concerned with social implications of birth control. The latter section contains much controversial material which the author frequently handles with a pleasant, if not always convincing assurance. The work includes much statistical data, a lengthy bibliography, an index and three appendices dealing with (a) Statistics of patients' social status, (b) Present and colonial fertility, (c) Additional centers for birth control. Dr. Robert L. Dickinson contributes a Foreword.

POPULATION (*Lectures on the Harris Foundation 1929*).
By Corrado Gini, Shiroshi Nasu, Robert R. Kuczynski, and Oliver E. Baker.
University of Chicago Press
$3.00 5 x 7½; ix + 312 *Chicago*

In this series of lectures Professor Gini develops his concept of the nation as a superorganism with its periods of birth, growth, senescence and death, or in some cases rebirth; Professor Nasu discusses population and the food supply, with an interesting section on Japanese population problems; Dr. Baker treats the trend of agricultural production in North America, concluding that the great problem facing the American farmer will continue to be how to dispose of the surplus; and Dr. Kuczynski gives the reasons for his conclusion that the peoples of Western Europe and North America are close to a stationary condition.

THE MOUND-BUILDERS. *A Reconstruction of the Life of a Prehistoric American Race, Through Exploration and Interpretation of Their Earth Mounds, Their Burials, and Their Cultural Remains.*
By Henry C. Shetrone. D. Appleton and Co.
$7.50 6 x 9¼; xx + 508 *New York*

A highly interesting account of "the First American" written for the non-specialist. The author, curator of the museum containing the finest collection of relics of these early settlers, gives a remarkably clear picture of what investigations by archaeologists and anthropologists have produced. The numerous and well chosen illustrations add materially to the pleasure of reading the book. The bibliography contains general references on anthropology, the American Indian, archaeology of the Mound Area, and under headings of states where mounds have been studied, special references. There is an excellent index.

MENSCHEN DER VORZEIT. *Ein Überblick über die altsteinzeitlichen Menschenreste.*
By Hans Weinert. Ferdinand Enke Verlag
Rm. 8 (paper) *Stuttgart*
Rm. 9.50 (cloth)
6½ x 10; 139

A presentation, so far as possible in non-technical terms, of what is really known about paleolithic man, without attempts to embellish the subject with imaginary additions.

RASSENKUNDE DES JÜDISCHEN VOLKES.
By Hans F. R. Günther.
J. F. Lehmanns Verlag
11 marks (paper) *München*
13 marks (cloth)
6 x 8⅞; 352

The kernel of this book was an appendix to the earlier editions of Dr. Günther's *Rassenkunde des deutschen Volkes*. His conclusion is that the Jews are not a race but a mixture of races.

ANCIENT EMIGRANTS. *A History of the Norse Settlements of Scotland.*
By A. W. Brøgger. Oxford University Press
$5.00 5½ x 8⅜; xi + 208 *New York*

Supplementing the scanty written records with evidence from place-names and archaeology, Dr. Brøgger attempts to reconstruct the history of the Norse settlements of the Orkneys and Shetlands.

ZOOLOGY

THE SALAMANDERS OF THE FAMILY PLETHODONTIDAE.
By Emmett R. Dunn. Smith College
$6.00 *Northampton, Mass.*
6¼ x 9½; x + 441

To any person who shares the popular superstition that a taxonomist is not a human being, but merely a complex kind

of classifying machine, we recommend the reading of the Foreword to this book, with its vivid glimpses of places visited and adventures encountered in the quest for salamanders.

It has been thirteen years since the eighteen-year-old boy read those few words and in those thirteen years for him the surf has whitened the shores of Caribbean islands; the slopes of the Balsams have been blue in the distance; hawks have soared a thousand feet below the naked peak of Sharp Rock; the iridescent wings of Morpho have fluttered through glades in the rain forest and in the midst of the cataract at Xico; from the mooring at Vera Cruz tall Orizaba has stood against the western sky; the low sun has shone on the ice of Ixtaccihuatl, most unforgettable of mountains; step after step, for him, far out at sea, Chirripo Grande, "*nie von Menschenfuss betreten*," has climbed aloft; and from shaken Irazu, while the ash cloud of the eruption rose above his head and floated, a black pall beaten by the fierce wind, he has seen through a break in the clouds, far to the Southwards, the sheer and menacing shaft of rock which is Cerro de la Muerte.

So far as we can see, Dr. Dunn does not suffer as a taxonomist for being something of a poet as well: his monograph is a model of careful description and classification. We wish, however, that he would give us some day a fuller narrative of his adventures as a collector. It is an old and honorable form of naturalistic literature, and the book should be worth reading.

THE WILDERNESS OF DENALI. *Explorations of a Hunter-Naturalist in Northern Alaska.*
By Charles Sheldon. Charles Scribner's Sons
$6.00 6⅛ x 9⅛; xxv + 412 *New York*

The author of this entertaining account of a search for white or Dall sheep in the wildernesses of the region of Mt. McKinley died before his material was in final form for printing. This explains the wide gap in time between the years of exploration and hunting (1906–1908) and the recent date of publication. Readers of this type of literature will derive much pleasure from the account of one who was not only a big game hunter but an explorer and naturalist as well. He has contributed many important facts concerning the habits and behavior of Alaskan mammals, especially those of the larger type, and in particular of the mountain sheep. The book is generously illustrated, contains temperature tables, lists of birds observed and mammals collected in the Denali region, localities in which the author hunted and tribes and bands visited. There is an excellent index and a map of the region explored. The explorer's wife contributes a preface and C. Hart Merriam an introduction. It is unfortunate that the picturesque name Denali, which the Indians gave the mountain, has not been retained.

THE WILD GRIZZLIES OF ALASKA. *A Story of the Grizzly and Big Brown Bears of Alaska, Their Habits, Manners and Characteristics, Together with Notes on Mountain Sheep and Caribou, Collected by the Author for the United States Biological Survey.*
By John M. Holzworth. G. P. Putnam's Sons
$5.00 6¼ x 9¼; xxi + 417 *New York*

Mr. Holzworth is an experienced naturalist and a pioneer in motion picture photography of big game in North America. The first part of his book is a narrative of his adventures in photographing Alaskan bears; the second part is a description of the grizzly and brown bears, with a plea for their protection from extermination. The illustrations are superb. We can heartily recommend this book, both to those interested in bears and to those who like their adventure stories authentic as well as thrilling.

HUNTING THE ALASKA BROWN BEAR. *The Story of a Sportsman's Adven-*

ture in an Unknown Valley After the Largest Carnivorous Animal in the World.
By John W. Eddy. *G. P. Putnam's Sons*
$3.50 6 x 8¾; xv + 253 *New York*

An account of a shooting expedition; not particularly interesting except to head and skin hunters. The illustrations of wild living bears and caribou were hardly worth including. There are some observations on the habits of the brown bear, but little of great value.

VERTEBRATE NATURAL HISTORY OF A SECTION OF NORTHERN CALIFORNIA THROUGH THE LASSEN PEAK REGION. *University of California Publications in Zoology Volume* 35.
By Joseph Grinnell, Joseph Dixon and Jean M. Linsdale.
University of California Press
$6.00 *Berkeley*
7 x 10¾; v + 594 (paper)

This is the seventh in a series of surveys made by the California Museum of Zoology on vertebrate animal life in California. The "Lassen section" is in the northern part of California, where profound changes are taking place due chiefly to the invasion of man. The report includes records on the species and subspecies of land vertebrates found; the frequency of observed occurrence and the relative abundance of these kinds; the local or habitat distribution of each kind; factors determining distribution, etc. The work is abundantly illustrated, chiefly with photographs, contains a list of references and is indexed.

THE AFRICAN REPUBLIC OF LIBERIA AND THE BELGIAN CONGO. *Based on the Observations Made and Material Collected During the Harvard African Expedition, 1926-1927. Vols. I and II.*
Edited by Richard P. Strong.
Harvard University Press
$15.00 a set *Cambridge*
7½ x 10⅜; xxxv + 1064

Two large and handsome volumes on the results of the Harvard African Expedition of 1926-27, which contain material of interest to students of many branches of science. Part I of Volume I contains an account of Liberia and its natives; Parts II and III deal with medical, pathological, and biological investigations. Volume II contains the zoological results of the expedition.

EXPERIMENTAL-ZOOLOGIE. 7. ZOOTECHNIKEN. *Eine Zusammenfassung der für Versuche mit Tieren Verfügbaren Forschungsweisen. (Fragestellung, Versuchsführung, Bearbeitung).*
By Hans Przibram. *Franz Deuticke*
30 marks (paper) *Leipzig and Vienna*
45 marks (bound)
7 x 10¼; viii + 269 + 10 plates (paper)

The concluding volume of this work deals with the technique of experimental zoology. The three sections deal with preparation for experiment, the actual experimental work, and the preparation of the results for publication. There is much valuable advice on all sorts of subjects; not the least valuable are the chapters on sources of errors.

HISTOLOGICAL AND ILLUSTRATIVE METHODS FOR ENTOMOLOGISTS.
By H. Eltringham (With a chapter on mounting whole insects by H. Britten).
Oxford University Press
$2.50 4¾ x 7⅞; xii + 139 *New York*

A useful addition to the literature of laboratory technique.

This little handbook does not aspire to be a textbook of Invertebrate Histological Technique. Indeed the subject can hardly be regarded as sufficiently advanced for the production of a volume in any way comparable to the available manuals on the staining and preparation of vertebrate tissues. It is rather an attempt to describe, for the use of those who have not had the advantage of laboratory training, the more elementary methods of entomological section-cutting and staining together with hints on other processes of microscopic entomology.

INVESTIGATIONS ON PLANKTON PRODUCTION IN FISH PONDS. *Bureau of Fisheries Document No. 1082*
By A. H. Wiebe.
U. S. Government Printing Office
15 cents 7½ x 11; 40 (paper) *Washington*

An account of experiments to determine the factors involved in the use of fertilizers to increase the production of plankton as food supply for fish. Soluble phosphorus is sometimes a limiting factor, but apparently there are also other limiting factors, as yet not identified.

THE STUDY OF BIRDS. *An Introduction to Ornithology.*
By E. M. Nicholson.
Jonathan Cape and Harrison Smith
60 cents 4½ x 6¾; 125 *New York*

The purpose of this little book is to outline "the principal methods by which ornithology is at present proceeding, and the problems which it is solving, or hopes to solve in the future." It will be of interest to amateur naturalists and to members of ornithological clubs.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 338.* Containing following articles: *Die pH-Bestimmung mit dem Hydrionometer*, by Ernst Bresslau; *Haltung und Aufzucht von Süsswasserschwämmen*, by Kurt Schröder; *Fang, Pflege und Zucht der deutschen Süsswasseregel*, by Konrad Herter.
Urban und Schwarzenberg
4 marks 7 x 10; 68 (paper) *Berlin*

This number of the Abderhalden *Handbuch* contains a good discussion of the laboratory husbandry of fresh-water sponges and leeches.

COSTUMBRES DE INSECTOS. *Observadas en Plena Naturaleza.* Tomo I. Tomo II.
By P. Eugenio Saz, S. J. *Revista Ibérica*
2 ptas. each *Barcelona*
6 x 8½; Tomo I, 103 (paper)
Tomo II, 95 (paper)

In these volumes a Spanish Father Wasmann records his observations on insects and applies them *ad majorem Dei gloriam.*

CILIATES FROM BOS INDICUS LINN. *I. The Genus Entodinium Stein. University of California Publications in Zoology, Vol. 33, No. 22.*
By Charles A. Kofoid and Ronald F. MacLennan.
$1.00 7 x 10¾; 74 + 4 plates (paper)

THE MORPHOLOGY, TRANSMISSION, AND LIFE-HISTORY OF HAEMOPROTEUS LOPHORTYX O'ROKE, A BLOOD PARASITE OF THE CALIFORNIA VALLEY QUAIL. *University of California Publications in Zoology, Vol. 36, No. 1.*
By Earl C. O'Roke.
University of California Press
65 cents *Berkeley*
7 x 10¾; 50 + 2 plates (paper)

BOTANY

MOLDS, YEASTS, AND ACTINOMYCETES. *A Handbook for Students of Bacteriology.*
By Arthur T. Henrici.
John Wiley and Sons, Inc.
$3.50 5¾ x 9⅛; x + 296 *New York*

A book of great usefulness to students and investigators. Molds, yeasts, and

actinomycetes cause diseases in man and animals; chemical transformations of the soil; and contamination of food. They are valuably employed in a wide variety of industries and in the future will probably be more so. These considerations lead the author to the view that these organisms require a more extensive general treatment than has hitherto been accorded them. The present volume is the outgrowth of a course of lectures given at the University of Minnesota to advanced students whose future work was to be in bacteriology. The author therefore deals equally with the medical and industrial aspects of the subject. The treatment is such that students, and bacteriologists in particular, can easily use the more technical literature. The book is generously illustrated. The keys are not too detailed but sufficient for advanced work. Literature lists accompany each chapter and there are author and subject indices.

EXPLORING FOR PLANTS. *From Notes of the Allison Vincent Armour Expeditions for the United States Department of Agriculture, 1925, 1926, and 1927.*
By David Fairchild. The Macmillan Co.
$5.00 6 x 9¼; xx + 591 *New York*

The author of this delightful book has succeeded in conveying to the reader much of the excitement and pleasure which he experienced in his search for plants in foreign lands. As a special agricultural explorer of the Office of Foreign Plant Introduction of the United States Department of Agriculture, Dr. Fairchild searches for rare and useful plants that can be introduced into the United States either for the purpose of improving existing strains or to increase our useful and ornamental varieties. Algeria, Morocco, the west coast of Africa, Ceylon, Sumatra and Java were his hunting grounds in this volume. In many of the regions visited he found much material in botanic gardens. These ranged from small private jungle gardens to the formal and very beautiful Peradeniya garden in Ceylon, established by the British in 1821. Much of the collecting, however, was in the wild. It was inevitable that there should be many unique experiences which the ordinary traveller in the orient misses, but throughout the book the reader's main interest is linked with the author's in the search for new and interesting plants. The book is abundantly illustrated with photographs taken on the expeditions. There is an index.

THE GREEN LEAF. *The Major Activities of Plants in Sunlight.*
By D. T. MacDougal. D. Appleton and Co.
$2.00 5 x 7½; iv + 142 *New York*

Among the most promising of the series of books on science for the general reader which have been appearing lately is Appleton's *New World of Science* Series. This was inaugurated by Heyl's excellent *New Frontiers of Physics*, one of the most lucid accounts that we have read of the subject, and also includes Taylor's *Antarctic Adventure and Research*, already noticed in these columns, and Koppanyi's *The Conquest of Life*. The present book deals with plant physiology, a subject about which most of us know much less than about animal physiology. We can heartily recommend it to all those who like their popular science to be accurate as well as picturesque.

OUR PLANT FRIENDS AND FOES.
By William A. DuPuy.
The John C. Winston Co.
$1.00 5¼ x 7¼; xiv + 277 *Philadelphia*

An entertaining and instructive book for the amateur. There is much in it that

is common knowledge to the student and the investigator but is not usually recounted for the layman in non-technical language. We read that tomatoes grow on poisonous plants; that although bamboo grows to be 100 feet tall, it is a grass; that the apple tree is an overgrown rose bush, that the development of the sugar beet industry goes back to the Napoleonic Wars when Napoleon ordered French investigators to find a substitute for sugar cane, etc., etc. There is a very clear exposition of the manufacturing of sugar by green leaves. Great care has been taken to make the book authentic. It is well illustrated but is without index.

STRASBURGER'S TEXT-BOOK OF BOTANY. *Sixth English Edition.*
Rewritten by Hans Fitting, Richard Harder, Hermann Sierp, George Karsten. Translated from the Seventeenth German Edition by W. H. Lang. *The Macmillan Co.*
$9.00 5¾ x 8½; xi + 799 *New York*

The first edition of this widely used text book first appeared in 1894. Since then it has gone through many editions, both in German and in English, and there have been extensive changes made involving complete rewriting. The original authors no longer appear on the title page and only the founder's name has been preserved in the title. In the present edition the section on *Morphology* is by Fitting; that on *Physiology* by Sierp; that on *Thallophyta, Bryophyta, and Pteridophyta* by Harder; and that on *Spermatophyta* by Karsten.

A TEXTBOOK OF PLANT PHYSIOLOGY.
By N. A. Maximov (Translated from the Russian. Edited by A. E. Murneek and R. B. Harvey). *McGraw-Hill Book Co., Inc.*
$4.00 5¾ x 9; xvi + 381 *New York*

A welcome addition to text and reference books in plant physiology. The author, director of plant physiological work in the Institute of Applied Botany of the Soviet Government, is outstanding for his work on temperature, light and water relations in plants. The book is well arranged for teaching. Frequent use has been made of American contributions, but the chief feature of the book is the opportunity which it offers to those who do not read Russian to become familiar with important researches, particularly on cold and drouth resistance, which the author has made his particular field. The illustrations are excellent and well chosen. There is a bibliography of English titles and an index.

The translation has been made with much care. All corrections which are to be incorporated in the forthcoming second Russian edition appear in this issue.

PLANT PHYSIOLOGICAL CHEMISTRY.
By Rodney B. Harvey. *The Century Co.*
$6.00 5¾ x 8¾; xix + 413 *New York*

A text book on the physiological chemical mechanism of plants. The main headings are: Introduction; General metabolism; Carbohydrates; Fats, phosphatides, and waxes; Proteins; Photosynthesis; Respiration. There are practically no references to the literature in the text, but a bibliography of twenty-two pages is appended.

THE LOWER FUNGI. *Phycomycetes.*
By Harry M. Fitzpatrick.
McGraw-Hill Book Co., Inc.
$4.00 5¾ x 9; xi + 331 *New York*

This book will serve as a text-book or reference work for beginning and advanced students. It has been designed also to provide a comprehensive treatment of the taxonomy and morphology of the Phyco-

mycetes for the research worker. The book is copiously illustrated and contains keys to all the genera. Literature lists, frequently lengthy, are included in each section. There is an index.

GENERAL ELEMENTARY BOTANY. *With Practical Applications. Revised Edition.*
By Elmer Campbell. Thomas Y. Crowell Co.
$3.00 5½ x 8½; xiii + 410 *New York*

MORPHOLOGY

LEONARDO DA VINCI THE ANATOMIST (1452-1519).
By J. Playfair McMurrich.
Published for Carnegie Institution of Washington by The Williams and Wilkins Co.
$6.00 6⅞ x 9⅝; xx + 265 *Baltimore*

This beautiful volume reflects great credit upon its author; upon his sponsor, the Carnegie Institution of Washington; and upon its publisher. It is a full-length portrait of Leonardo as a biologist. It is equally characterized by literary charm and sound, penetrating scholarship. Once more it brings evidence of the rightness of the thesis we have before maintained in these columns, that the professional scientific man is apt to become a more effective historian of his subject than is the professional historian who turns to science for material with which to exercise his pen and his scholarship.

The first six chapters lay the historical background for the discussion of Leonardo's anatomical work. The next twelve chapters take up the details of that work, by organ systems. These are followed by three chapters on embryology, comparative anatomy and botany, respectively, while a short final chapter of conclusions brings the text to an end. There is a bibliography and a detailed index. The volume is abundantly and well illustrated.

McMurrich accords Leonardo a high place as an anatomist, which would have been higher had his work been published. He says: "Vesalius was undoubtedly the founder of modern anatomy—Leonardo was his forerunner, a St. John crying in the wilderness."

CONTRIBUTIONS TO EMBRYOLOGY. *Volume XXI, Nos. 118 to 125. Carnegie Institution of Washington Publication No. 407.* Containing following articles: *A Human Embryo with Seventeen Pairs of Somites*, by Wayne J. Atwell; *Description of a Human Embryo of Eight Somites*, by Cecil M. West; *Medullated Tracts in the Brain Stem of a Seventh-Month Fetus*, by O. R. Langworthy; *Ossification of the Otic Capsule in Human Fetuses*, by T. H. Bast; *On an Unusual Placental Form in the Hyracoidea; Its Bearing on the Theory of the Phylogeny of the Placenta*, by George B. Wislocki; *Gross and Microscopic Structure of Thyroid Gland in Man*, by W. F. Rienhoff, Jr.; *The Age Factor in Grafts*, by Vera Danchakoff and V. E. Danchakoff; *The Early Embryology of the Rabbit*, by P. W. Gregory.
Carnegie Institution of Washington
$6.00 (paper) *Washington*
$7.00 (cloth)
9¼ x 11½; 168 + 29 plates

Another volume of these contributions, which maintains, both in matter and in form, the high standard set by previous volumes.

STUDIES ON THE STRUCTURE AND DEVELOPMENT OF VERTEBRATES. *By Edwin S. Goodrich. The Macmillan Co.*
$10.00 5⅜ x 8½; xxx + 837 *New York*

A careful and useful treatise on some problems of vertebrate morphology. The chapter headings indicate the scope of the

work: Vertebral column, ribs and sternum; Median fins; Paired limbs; Limb girdles; Morphology of head region; The skull; Skeletal visceral arches and labial cartilages; Middle ear and ear ossicles; Visceral clefts and gills; Vascular system and heart; Air-bladder and lungs; Subdivisions of the coelum, and diaphragm; Excretory organs and genital ducts; Peripheral nervous system and sense organs. The illustrations are numerous and excellent; over 300 out of 754 are new. There is a classification scheme of 16 pages; a bibliography of 1186 titles, and a good index. A sound and valuable work.

A TEXT-BOOK OF HISTOLOGY.
By Alexander A. Maximow. Completed and edited by William Bloom.
W. B. Saunders Co.
$9.00 6½ x 9¾; xiii + 833 *Philadelphia*

It was Professor Maximow's plan to write a text-book based, so far as possible, on human material. At the time of his death he had entirely completed the sections on the male and female generative organs, the urinary tract, the organs of special sense, and epithelium. Others, on the blood and connective tissue, the gastro-intestinal tract, the blood vascular and lymphatic systems, the spleen, the integument, and the mammary gland, were in rough manuscript. The difficult task of completing the book fell to Professor Bloom. This he has done with careful regard to Professor Maximow's original plan. The sections on the biliary and respiratory systems, the pancreas, and the endocrine glands, and also the introductory chapter, have been written by him, and he has revised and completed those parts that were in rough draft. Professor Herrick is chiefly responsible for the section on the nervous tissue.

The illustrations are numerous and excellent, the references are few but have been carefully selected. There is a detailed index. Teachers of histology will find this an excellent text-book. It should be among the reference books in all biological laboratories.

THE ANATOMY OF THE HUMAN BODY. *Twenty-second Edition.*
By Henry Gray (Thoroughly Revised and Re-edited by Warren H. Lewis).
Lea and Febiger
$10.00 6½ x 10⅛; 1391 *Philadelphia*

While the latest edition of this classic text has been brought up to date by incorporating new anatomical knowledge and by a thorough revision of the section on the central nervous system, "the plan originally formulated, which has proved so successful, has been adhered to as much as possible."

A TEXT-BOOK OF HISTOLOGY.
By Harvey E. Jordan. D. Appleton and Co.
$7.00 6 x 8½; xxviii + 857 *New York*

The fifth edition of this excellent text has been thoroughly revised and added to, especially in the chapters on blood and the endocrine tissues.

DEVELOPMENTAL ANATOMY. *A Text-Book and Laboratory Manual of Embryology. Second Edition. Reset.*
By Leslie B. Arey. W. B. Saunders Co.
$6.50 6½ x 9¾; ix + 563 *Philadelphia*

GRUNDZÜGE DER ENTWICKLUNGSGESCHICHTE DES MENSCHEN *in vergleichender Darstellung. 12. neubearbeitete und wesentlich erweiterte Auflage.*
By Richard Weissenberg. Georg Thieme
15 marks *Leipzig*
5 x 7¾; xv + 437 + 6 plates

PHYSIOLOGY AND PATHOLOGY

THE STORY OF A SURGEON.
By Sir John Bland-Sutton.
Houghton Mifflin Co.
Boston and New York
$3.50 5½ x 8¾; xii + 204

Sir John Bland-Sutton is one of the most picturesque figures in the medical world. To an extraordinary degree he has kept undiminished, throughout a long and distinguished career as a surgeon, that naive intellectual curiosity and wide-ranging interest in literally everything, which is commonly found in a bright and keen boy, but almost never in a man of 76 loaded with honors and professional and social responsibilities. This autobiography is a delightful book. One's only regret is that it is not twice as long. As those who know him personally are aware, and as is reflected in this book as well as in all his other writings, Sir John's store of odd and curious information, especially about zoological matters, is simply amazing. Zoology was his first love, and he has always been faithful to it. He says (p. 178):

> Zoology is omnipotent among sciences and it is the oldest study in the world. We become familiar with it in the nursery, and our interest in it increases with our religious exercises. Eden was the first Zoological Gardens and Adam had the privilege of naming animals unfettered by priority of nomenclature which is the bugbear of learned zoologists. The Bible opens with a delightful jungle story: Eden—a jungle—where our first parents wandered in perfect happiness until they were disturbed by an Evil Spirit in the form of a wily snake under an apple-tree, and Eve, wishful for a change of diet, ate an apple, lost Eden and brought Sin and Death into the world.

Every biologist should read this book. It will stimulate as well as entertain him. It is copiously illustrated, including some examples of Sir John's own skill as a draughtsman—he says that he learned to draw by watching pavement artists. Unfortunately there is no index.

NERVOUS INDIGESTION.
By Walter C. Alvarez. *Paul B. Hoeber, Inc.*
$3.75 5½ x 8⅛; xvi + 297 *New York*

To the general practitioner this book will be invaluable. Much common sense and sage advice are to be found within its pages. The author is on the staff of the Mayo Clinic. He is a physician of wide experience in the handling of nervous patients and has an unusually keen appreciation of the difficulties which beset the person with functional disorders of the digestive tract as well as the difficulties which confront the physician in treating these disorders. The subjects discussed are: Ways in which emotion can affect the digestive tract; Types of indigestion; Hints in regard to the taking of a history; The handling of the nervous patient; The treatment of nervous indigestion; Some practical points about the physiology and innervation of the digestive tract; Suggestions for further reading. In the preface Dr. Alvarez makes a plea for a more extensive training of the medical student in the treatment of the nervous patient. The book is well documented and indexed.

A TEXT-BOOK OF PHYSIOLOGY *for Medical Students and Physicians. Eleventh Edition. Thoroughly Revised.*
By William H. Howell.
W. B. Saunders Co.
$6.50 5¾ x 9⅛; 1099 *Philadelphia*

In the latest revision of this standard text-book Dr. Howell follows his previous policy of selection rather than condensation, of treating the more important subjects in sufficient detail to give the student a serviceable conception of the facts, in-

stead of attempting a more summary treatment of a larger number of topics.

Everything that can be ascertained in regard to the general physiology of the cell is of fundamental importance to the student of physiology, because in the long run explanations of the phenomena exhibited by living things must rest upon these basic properties, but we must recognize that in the multicellular organisms differentiation in function and the mutual dependence and interaction of the various tissues, in carrying out the reactions of the complex structure considered as a whole, have given rise to numerous delicately balanced mechanisms that are of essential importance for the maintenance of the integrity of the organism. The immediate need of the medical student is to become conversant with these mechanisms as far as they are known, and, perhaps, the first duty of a text-book in physiology is to give the necessary information as clearly and accurately as possible.

In this sense a text-book of animal physiology is still largely descriptive in its treatment, although as our knowledge becomes more exact and more comprehensive mere description is being replaced or supplemented in large measure by expositions of fundamental principles.

EDWARD JENNER *and the Discovery of Smallpox Vaccination.*
By Louis H. Roddis. George Banta Publishing Co.
$1.00 5 x 7; iv + 155 *Menasha, Wis.*

Although a small book there is much interesting material to be found within these pages. Jenner led a busy life as a country practitioner but he had many other interests than healing the sick. Music gave him great pleasure, he sometimes composed verse, also he devoted considerable time to the study of natural history, being especially interested in the migratory habits of British birds. The author gives a graphic description of Jenner's work on small pox and of the interest and antagonism which it aroused among practitioners and laymen. Many of Jenner's letters and those of his correspondents are included in the text and several interesting documents among which are "Some accounts of the success of inoculation for the small pox in England and America," by Benjamin Franklin; "Plain instructions for inoculation in the small pox," by William Heberden, and William Cobbett's diatribe on vaccination. The work concludes with a list of 32 important contemporary books and documents referring to small pox, a list of Jenner's published writings and an appendix in which are included reports of various vaccination commissions, mortality tables, etc.

PHYSIOLOGY.
By John F. Fulton. Paul B. Hoeber, Inc.
$1.50 $4\frac{1}{4}$ x $6\frac{5}{8}$; xvi + 141 *New York*

An excellent addition to the *Clio Medica* series of historical primers published by Paul Hoeber. In five chapters and an Epilogue, the author, who has recently become Stirling Professor of Physiology in Yale University, discusses Aristotle and Galen; the circulation of the blood; respiration; digestion; physiology in the nineteenth century and the rise of teaching laboratories; and the trend of modern physiology. Having regard to the limitations imposed by its size it seems to us to be the best history of physiology yet written. There is a good bibliography and thorough indexes.

AN INTRODUCTION TO HUMAN EXPERIMENTAL PHYSIOLOGY.
By F. W. Lamb. Longmans, Green and Co.
$4.00 $5\frac{3}{8}$ x $8\frac{1}{2}$; xii + 335 *New York*

Undoubtedly the study of physiology with the student himself or his fellow-student as the subject of investigation has much to commend it. In this book is presented "the class experiments on blood, respiration, and circulation, and in some sections more advanced work, together

with remarks on the problems to which the methods have been applied by various workers." The book is well documented and indexed.

ALIMENTARY ANAPHYLAXIS (*Gastro-intestinal Food Allergy*).
By Guy Laroche, Charles Richet Fils and François Saint-Girons. Foreword by Charles Richet. Translated by Mildred P. Rowe and Albert H. Rowe.
University of California Press
$2.00 postpaid 5 x 7; 139 *Berkeley*

Many interesting facts are brought out in this little book indicating the importance and need of further research into the causes, symptoms and treatment of food allergy. The authors believe that all foods, except water and sugar, and most chemical products may produce an anaphylactic state. The discussion of the experimental work relates almost entirely to that of European investigators. There is no mention of the work in skin reactions, which is essentially an American contribution. One of the translators contributes a preface to the American edition. There is a bibliography of over 100 titles but no index.

ARTIFICIAL SUNLIGHT. *Combining Radiation for Health with Light for Vision.*
By M. Luckiesh.
D. Van Nostrand Co., Inc.
$3.75 6 x 9; 254 *New York*

A treatise on the artificial production of the various forms of radiation found in sunlight. There is a considerable amount of technical data which will be useful to anyone who desires to do experimental work on the biological aspects of radiation.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 331. Methoden und Technik der vergleichenden Stoffwechselphysiologie bei Wirbellosen.*
By H. P. Wolvekamp.
Urban und Schwarzenberg
5.50 marks 7 x 10; 86 (paper) *Berlin*

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 332. Methoden zur Untersuchung des Verwendungsstoffwechsels pathogener Bakterien.*
By Hugo Braun *Urban und Schwarzenberg*
4.20 marks 7 x 10; 78 (paper) *Berlin*

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 341.* Containing following articles: *Die radiometrische Mikroanalyse*, by Rudolf Ehrenberg; *Eine Methode zur Beobachtung lebender Organe mit stärksten Vergrösserungen im Lumineszenzlicht (Intravitalmikroskopie)*, by Philipp Ellinger and August Hirt; *Die Methoden der elektrischen Kurzwellenanwendung*, by Erwin Schliephake; *Methodik der Messung kurzschenkeliger Winkel an biologischen Objekten*, by Paul Weiss.
Urban und Schwarzenberg
9 marks 7 x 10; 112 (paper) *Berlin*

These numbers of the Abderhalden *Handbuch* are of interest to physiologists. Number 331, which completes the volume on the comparative physiology of invertebrates, and deals with invertebrate metabolism, will be of general interest.

CLINICAL AND EXPERIMENTAL EXAMINATIONS IN PATIENTS SUFFERING FROM MB. MENIÈRI INCLUDING A STUDY OF THE PROBLEM OF BONE-CONDUCTION. (In two volumes).
Acta Oto-Laryngologica. Supplementum X, XI.
By Dida Dederding
Mercators Tryckeri Aktiebolag, Finland
(May be obtained through The

Editor, Sabbatsbergs Sjukhus, Stockholm)
25 Swedish crowns per volume
(4 fascicles)
6⅜ x 9½; No. I, 156
No. II, 213 (paper)

An investigation of 135 patients in the Municipal Hospital of Copenhagen, with full clinical and experimental details. "Mb. Menièri" is the disease which has as one of its symptoms a continuous ringing in the ears. Martin Luther is supposed to have had it. Indeed it has been alleged to have been the cause of some of the sourer elements of his theology.

BIOCHEMISTRY

ENZYMES.
By J. B. S. Haldane.
Longmans, Green and Co.
$5.50 6 x 9½; vii + 235 *New York*

This book is based on a course of lectures which the author has been giving in the University of Cambridge for the last seven years. There has been no attempt to present a comprehensive treatise on the subject, the author having limited himself to an account of the purely chemical side of enzymes. It is necessary that the reader have a considerable knowledge of organic and physical chemistry. After a brief introduction enzymes are discussed under the following headings: The influence of enzyme concentration and hydrogen ion concentration; The union of the enzyme with its substrate and related compounds; The influence of temperature and radiation on enzyme action; The course of enzymatic reactions, and its mathematical theory; Specificity; Coenzymes, activators, kinases, and complements; The poisoning of enzymes; The purification and chemical nature of enzymes; Theories of enzyme action, and classification of enzymes. There is appended to Chapter VI a brief section on recent work in carbohydrate chemistry. The book is well documented, and maintains the high standard of quality associated with the series in which it appears.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 336. Die Verseifung.*
By Franz Bachér. Urban und Schwarzenberg
16 marks 7 x 10; 294 (paper) *Berlin*

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 339. Neuere Synthesen biologisch wichtiger Pyridinkörper.*
By Hans Horsters and Helene Horsters.
Urban und Schwarzenberg
5 marks 7 x 10; 92 (paper) *Berlin*

These two numbers of the Abderhalden *Handbuch* on saponification and on the synthesis of pyridine compounds will be of interest to biochemists.

SPIRAZINES. *A Type of Chemical Structure Bearing Upon the Constitution of Proteins and the Cause of Life.*
By Carl F. Krafft. *Carl F. Krafft*
50 cents *Box 1421, Washington, D. C.*
5 x 7; 54

Mr. Krafft suggests that the growth and division of cells may be explained by a spiral arrangement of atoms in the protein molecules, in place of the familiar ring. The idea is ingenious but it does not seem to have found favor with the biochemical journals.

SEX

THE DEVELOPMENT OF SEX IN VERTEBRATES.
By F. W. Rogers Brambell (with a Preface by Julian S. Huxley).
The Macmillan Co.
$4.00 5⅜ x 8½; xvi + 261 *New York*

The author has endeavored to bring together and correlate recent work on the development of sex, both from the morphological and physiological side, and from the genetic and cytological. The book is, taken as a whole, an excellent piece of work, and one that will provide a good survey of the present state of our knowledge. The chapter headings indicate the scope of the work: Reproduction in Vertebrates; Spermatogenesis; Oogenesis and Fertilisation; The Sex Chromosomes; The Sex-ratio; The Origin of the Primordial Germ-cells and the Formation of the Germinal Ridges; The Differentiation of Sex and the Development of the Gonads; The Ovarian Follicle; The Corpus Luteum; Hermaphroditism; Sex-reversal in Fish and Amphibia; Sex-reversal in Birds; Gynandromorphism and the Control of the Sexual Characters; Conclusion.

We must point out, however, one sentence: "It may be concluded that the study of the mammalian sex-ratio, probably the oldest method of attacking the problem of sex-determination, thus provides weighty evidence in favour of the sex-chromosome theory."

This sentence seems to us one of the worst examples of faulty reasoning we have recently seen. Actually, no solitary piece of evidence offered in the chapter on the sex ratio can be used in support of the sex-chromosome theory; every one, without exception, requires some additional *ad hoc* hypothesis to reconcile it with the theoretical consequences of the sex-chromosome theory. We do not argue that the theory is unsound; but we object to bad logic, and perhaps most strongly when it is used to support a good theory.

BIOMETRY

BARLOW'S TABLES *of Squares, Cubes, Square Roots, Cube Roots, and Reciprocals of All Integer Numbers up to 10,000. Third Edition.*
Edited by L. J. Comrie.
E. and F. N. Spon, Ltd.
7 s. 6 d. net *London*
5⅜ x 8½; xii + 208

Since 1840, when De Morgan edited a new edition of Barlow's Tables, successive reprints of this useful work have been printed from the same stereotype plates. As these, after ninety years of service, have begun to show wear, the publishers, in resetting the book, have taken the opportunity to introduce various improvements, such as interlinear differences and a column of $\sqrt{10n}$ to give the user the benefit of a four-figure argument in all extractions of a square root. Moreover, the tables of powers higher than the cube in the original edition of 1814, which were omitted in 1840, have been restored. With these improvements Barlow may look forward to another ninety years of usefulness to the quantitatively minded.

PSYCHOLOGY AND BEHAVIOR

THE PRINCIPLES OF PSYCHOPHYSIOLOGY. *A Survey of Modern Scientific Psychology. Volume II. Sensation.*
By Leonard T. Troland.
D. Van Nostrand Co.
$4.00 5½ x 8½; xxi + 397 *New York*

This is the second of four volumes planned to constitute a thorough treatment

of physiological psychology. The first volume deals with the definition of psychology, the methods of psychology, and perception; the latter subject being used to furnish a concrete and somewhat simple example of psychophysiological methods. The second volume concerns sensation, defined by the author as "the relationship between consciousness and processes in the afferent nervous arc, including the sense-organs and their immediate stimuli." The third volume is to be confined to a discussion of cerebration and action, while the fourth volume will consider the philosophical developments which arise from psychophysiology. Each one of the four volumes is so arranged that it can be read and used as a textbook without a knowledge of the other three.

Volume II, which deals with sensation, is

> concerned with the indirect relationships between consciousness and the so-called "afferent arc" or the sector of the nerve conduction path which starts with the sense-organ stimulus and ends at the sensory projection areas of the cerebral cortex.

In this study the author has brought together all the available knowledge from physiology and applied it to psychology. In the first chapter on the Concept of Sensation, a physiological interpretation of neural activity is clearly and thoroughly presented; the remaining chapters deal with visual sensation, auditory sensation, the chemical senses, the cutaneous senses, and kinaesthetic and internal sensations.

The author is of the opinion that advance in psychological theory will come only through a knowledge of the facts of psychophysiology; he discards extreme behaviorism as being worthless, and holds that introspective psychology is only of *value* when correlated with the facts which behaviorists are emphasizing. He also believes that the positive propositions of the *Gestalt* and structural psychologies are in general correct.

The book will make an excellent textbook for psychology students, because it will compel them to have a certain much-needed respect for physiology.

SOCIAL PSYCHOLOGY. *An Analysis of Social Behavior.*
By Kimball Young. F. S. Crofts and Co. $4.00 6 x 9¼; xxxviii + 674 *New York*

One of the most frequent criticisms of science is that, while it has vastly increased our control of the external world, it has given us little aid in controlling ourselves, either as individuals or as groups, that it has invented edged tools for our use but has not taught us how to use them without cutting ourselves. The most promising answer to this criticism seems to be the new science of social psychology, with which Dr. Young deals in this book. It is his contention that in the still somewhat embryonic stage of development of the subject the descriptive method is, on the whole, the most appropriate, leaving quantitative treatment and generalization for later workers. His book therefore uses the case study method extensively.

HUMANISTIC LOGIC FOR THE MIND IN ACTION.
By Oliver L. Reiser. Thomas Y. Crowell Co. $3.00 5½ x 8½; x + 326 *New York*

The author's *apologia* is:

> This book aims at being something more than just another logic text. It attempts to broaden the scope of logic by relating the reasoning process to other human activities. The purpose of a course in logic is not conceived to be that of making human beings over into purely intellectual reasoning machines. The function of thought is held to be that of coordinating responses, of integrating human interests. It is held, therefore, that the function of a course in logic consists in showing individuals how to achieve a measure

of intellectual synthesis, a unity of insight and outlook, in a world which is rapidly changing in its physical and social aspects.

The present volume is written under the definite conviction that before the logician can make persons logical he must get them interested in being as intelligent and as intellectually honest as they can be. Hence the first two parts of the book consist in an attempt to "humanize" logic, to relate it to the broader adventure of living in the modern world.

Dr. Reiser seems to us to have succeeded admirably in his aim. On some of the topics much more might have been said, but the book is an excellent introduction to logic from a biological and humanistic point of view.

BEHAVIORISM. *A Battle Line.* *Edited by William P. King.*

Cokesbury Press

$2.25 5½ x 8; 376 *Nashville, Tenn.*

Now that evolution has been put down, the next Holy War is evidently to be against behaviorism. On the jacket of this book we are warned that

Behaviorism in science and humanism in philosophy are the twin modern foes of religious faith.

In "Behaviorism: A Battle Line" representatives of Protestant, Catholic, and Jewish faiths take a common position against materialistic behaviorism. These noted clergymen, philosophers, editors, and psychologists make a merciless exposure of the errors of the new psychology both in theory and practice.

This book will awaken many to the danger that threatens. It will fortify them against the attack of the materialist who expresses the presumptuous purpose of destroying all religious faith.

A Cokesbury Good Book

However, the contents of the book are not nearly so dreadful as one might anticipate from this Encyclical, being devoted mainly to a criticism of the curious self-denying ordinance by which really devout behaviorists renounce the opportunity to peek behind the scenes of human behavior. We find little in it that has not been as well said before this.

MORAL SENSE.

By James Bonar. *The Macmillan Co.*

$4.00 5½ x 8½; 304 *New York*

In this volume of the Library of Philosophy, Dr. Bonar, the author of *Malthus and His Work*, traces "the rise, progress and decline of a theory of moral philosophy which prevailed in this country [England] for the greater part of the eighteenth century," a theory which held that we perceive that an act is right or wrong through a moral sense in much the same way that we perceive that an apple is sweet or sour through the sense of taste. Founded by Shaftesbury and developed by Hutcheson, it finally dissolved under the criticism of Kant. Shaftesbury's idea of the division of the self "into two parties," one judging the other, seems a foreshadowing of the Freudian Superego, but there is no mention in the book of Freud. Indeed, there is little to suggest that anything has happened in the world of thought since Hegel and his disciples.

A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY. *Volume I.*

By James M. Baldwin, Mary W. Calkins, Edouard Claparède, Raymond Dodge, Pierre Janet, Joseph Jastrow, F. Kiesow, William McDougall, Carl E. Seashore, C. Spearman, William Stern, Carl Stumpf, Howard C. Warren, Theodor Ziehen, H. Zwaardemaker. Edited by Carl Murchison.

Clark University Press

$5.00 6 x 9; xvii + 516 *Worcester, Mass.*

It is perhaps even truer in psychology than in other sciences that a knowledge of the history and personality of a worker aids one in understanding his work. It was therefore a happy thought of Professor Boring that led to this number of the *International University Series in Psychology*. Four volumes of autobiographies are planned, with additional volumes if

the idea proves fruitful. Among so many valuable accounts it is perhaps invidious to choose; we were, however, especially interested in those of Claparède and Seashore.

STAMMERING.
By Elsie Fogerty.
E. P. Dutton and Co., Inc.
95 cents 5 x 7¼; 64 *New York*

A brief discussion of the causes of stammering and methods of cure. The subject is one upon which we profess no competence; but the general impression we derive is that Miss Fogerty would probably be a good person to take charge of a stammering child.

THE MENTAL DEVELOPMENT OF THE CHILD. *A Summary of Modern Psychological Theory.*
By Karl Bühler. *Harcourt, Brace and Co.*
$3.00 5½ x 8½; xi + 170 *New York*

A brief account of the development of mental activity, interesting if not particularly startling. The description of the German child's acquisition of language indicates that our own ability in that language very closely approximates the average three-year-old.

The discussion of fairy tales we found interesting, but we missed any mention of the fact that children (at least some children) enjoy indefinite repetition of the same story; and they will insist on literal accuracy in the repetition.

SOCIAL PSYCHOLOGY. *The Psychology of Attraction and Repulsion.*
By John J. Smith. *Richard G. Badger*
$2.00 5 x 7⅞; xxv + 468 *Boston*

A dull collection of pious platitudes, supported by weak reasoning and unproved and unprovable assertions. There is a complete lack of any searching thought. The book may, as the author hopes, be useful to "Y. W. or Y. M. C. A. groups;" but we doubt its value to genuine students.

BEHAVIORISM. *Revised Edition.*
By John B. Watson.
W. W. Norton and Co., Inc.
$3.00 6 x 8⅜; xi + 308 *New York*

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

FADS AND FALLACIES IN PRESENT-DAY EDUCATION
By H. E. Buchholz. *The Macmillan Co.*
$1.50 (school edition) *New York*
$2.00 (trade edition)
5 x 7½; xiv + 200

The author of this delightful collection of essays is widely known, under the *nom de plume* of Ezekiel Cheever, as a trenchant and soundly realistic critic of current educational theories and practices. Several of the essays attracted much attention when they appeared in magazine form. Wit, humor, irony, and satire are effectively and entertainingly directed towards quackery, sham, and the uplift, as they nourish themselves in the field of education. We recommend the perusal of this book to everyone except school teachers and professors of pedagogy. They, poor things, would only have their feelings hurt, and we are opposed to cruelty to animals, even to those formerly designated at the University of Texas as "peedogs." The index is as entertaining as the rest of the book.

VIAGGI ED ESCURSIONI SCIENTIFICHE DI LAZZARO SPALLANZANI.

By Giacomo Pighini. *L. Cappelli*
Lire 40 *Bologna*
$7\frac{1}{4}$ x $9\frac{3}{4}$; xvii + 441 (paper)

The experiments on digestion and the controversy with Needham on spontaneous generation, by which Spallanzani is chiefly remembered, were by no means the only activities of the versatile and energetic *Abbate*. In the intervals between researches on a variety of subjects, he made journeys in the Alps and Appenines, along the Italian coasts and to the Levant. These were primarily to enrich the natural history collections at Pavia, but Spallanzani also made the most of his opportunity for biological and geological observations. The account of these journeys, from his unpublished journals as well as from previously published books, is given in this interesting volume.

SIR ISAAC NEWTON. 1727–1927. *A Bicentenary Evaluation of His Work. A Series of Papers Prepared under the Auspices of The History of Science Society in Collaboration with The American Astronomical Society, The American Mathematical Society, The American Physical Society, The Mathematical Association of America and Various Other Organizations.*

The Williams and Wilkins Co.
$5.00 $6\frac{1}{4}$ x $9\frac{3}{8}$; ix + 351 *Baltimore*

This collection of essays was brought together at the initiative of the History of Science Society, collaborating with various other scientific societies, on the occasion of the bicentenary of Sir Isaac Newton. The several papers vary in quality and importance as is, of course, inevitable in such a collection. The two which perhaps give the most novel light on Newton and his work are, first, that by George E. Roberts on "Newton in the Mint;" and, second, that by Frederick E. Brasch on "Newton's first critical disciple in the American Colonies—John Winthrop." Altogether the book is a valuable contribution to the literature of the history of science, and a credit to American scholarship. It is beautifully printed, but unfortunately lacks an index.

STUDIES IN THE LITERATURE OF NATURAL SCIENCE.

By Julian M. Drachman.
The Macmillan Co.
$4.00 $5\frac{3}{4}$ x $8\frac{1}{2}$; xi + 487 *New York*

As its title implies, this book is not a history of natural science from the usual point of view but a study of natural science as literature. Charles Darwin is the central figure; Lyell and Uniformitarianism on the one hand, Erasmus Darwin, Lamarck and the rest of the early evolutionists on the other, lead up to him. Dr. Drachman has a talent for neat phrases, as in his felicitous description of Owen as "the tenacious but doomed champion of a retreating Old Guard, an academic army that dies mentally but never surrenders to a new truth." From his remark on Darwin's *Movements and Habits of Climbing Plants:* "Though there are a few tables of statistics on kinds of plants and their rates of growth, much of the book is written so that it can be read with ease and pleasure," we infer that he does not think much of statistics as a form of literature.